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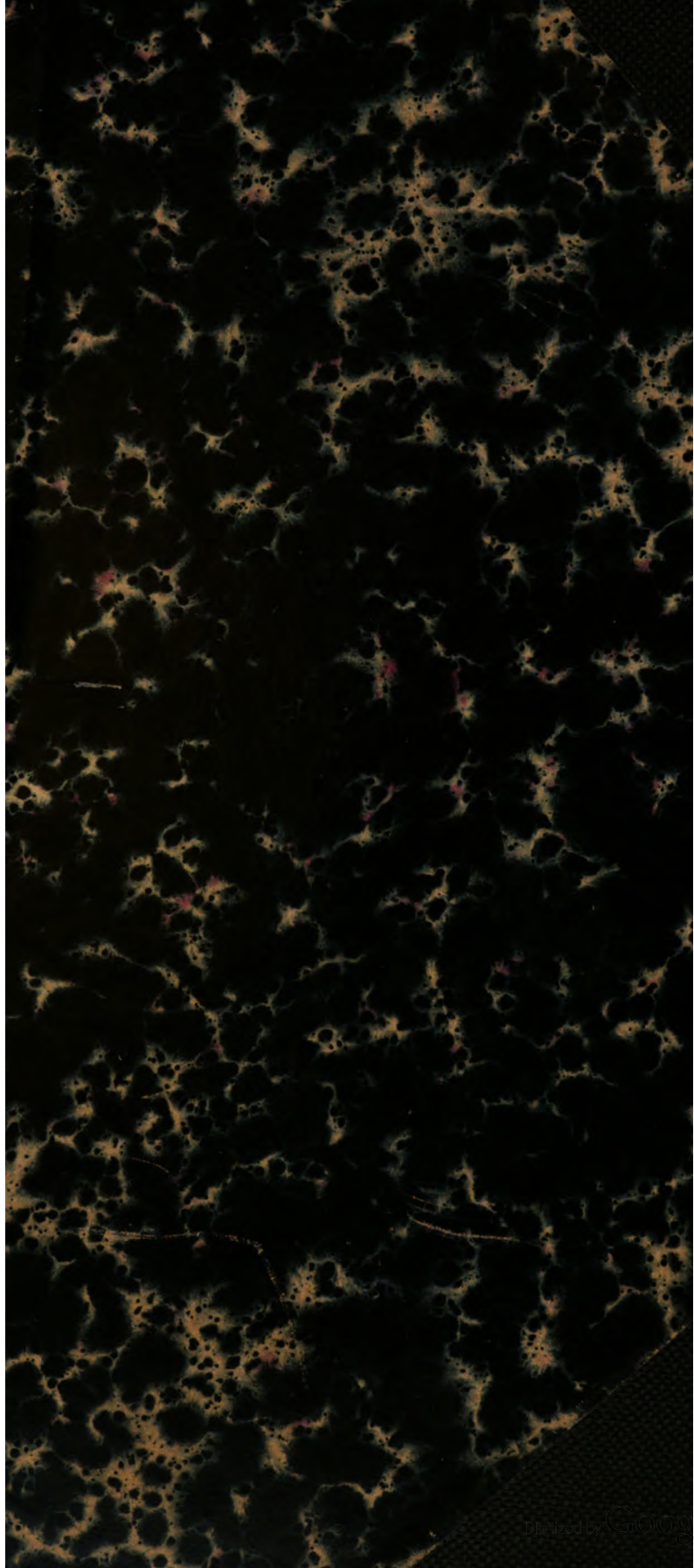
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PHILOSOPHICAL SOCIETY OF WASHINGTON

BULLETIN, Vol. XV, pp. 1-26

SAMUEL PIERPONT LANGLEY

BY

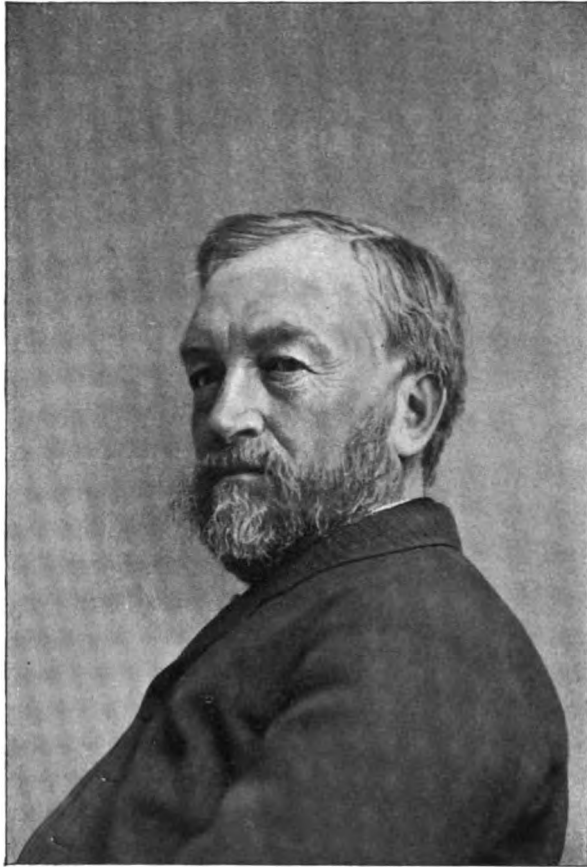
CYRUS ADLER

READ BEFORE THE PHILOSOPHICAL SOCIETY OF WASHINGTON

NOVEMBER 24, 1906

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S. P. Langley

SAMUEL PIERPONT LANGLEY.

BY

CYRUS ADLER.

[Read Before the Society, November 24, 1906.]

Samuel Pierpont Langley, the third Secretary of the Smithsonian Institution, astronomer and physicist, famed the world over for epoch-making contributions to our knowledge of the sun and the establishment of the principles of aërial flight, passed away in his seventy-second year, at Aiken, South Carolina, on the twenty-seventh of February, 1906.

Mr. Langley was descended of families which came to Massachusetts in the early part of the 17th century, and to a great extent remained in the colony and even in the State itself. In a biography prepared by the late George Brown Goode eleven years ago, it was pointed out that an unusual number of his ancestors were skilled mechanics and artisans, while, on the other hand, a group of them were of the most intellectual men of early New England—clergymen, school-masters, and indeed one of them, Increase Mather, a president of Harvard College and the author of the first American work upon astronomy.

His immediate forebears were especially characterized by great physical and intellectual vigor, wide cultivation, and a staunch sense of duty; and if to these distinguishing characteristics of a long line of ancestors, there be added mechanical skill, high moral ideals, and a restless, all-consuming pursuit of new truth, in season and out of season, by skillful methods, upon original lines, we have a picture of

the intellectual and moral make-up of the man whose life I am now attempting, inadequately, to portray.

He beguiled the tedium of his last illness by beginning the preparation of his memoirs, which I have been permitted to see. They are so fragmentary that they can never be published; but from them I have been able to learn a few incidents of his early life which it is not improper to recite.

He was born on the twenty-second of August, 1834, in Vernon street, Roxbury, to Samuel Langley and Mary Williams; attended various private schools, and later entered the Boston High School. His education was of the type then prevalent, and much of his time was devoted to Latin grammar. On the moral side, the two strongest impressions which he recollected of this period were being taught a horror of debt and, through it, a sense of duty; and these two traits were firmly present to the last.

Yet another fact taken from these very interestingly written pages shows that his father, himself a wholesale merchant in Boston, possessed a telescope with which the small boy watched the building of Bunker Hill monument.

As a child, he was an omnivorous reader, had a reflective mind, an interest in art and in foreign lands, and a very strong bent toward mathematics, all of which grew to importance in later life. Not being sent to college, his choice of a profession fell upon civil engineering and architecture, which were primarily chosen because they would afford a livelihood and at the same time keep him near to several of the studies that interested him most.

In 1857 he went to the West, and spent the next seven years mainly in Chicago and St. Louis, engaged in the practice of his profession and in business, acquiring a mercantile training and skill as a draftsman which were of high importance in his later scientific and administrative career.

In 1864 he definitely abandoned his profession and returned to New England, spending some time with his brother, John Williams Langley, in building a telescope; and the brothers afterward had a year or more of European travel, visiting art galleries and observatories, and indeed

all scientific institutions. This European journey had another notable influence in familiarizing him with the continental languages, especially French, in which he acquired great proficiency.

Upon his return to Boston, the then director of the Harvard College observatory, Professor Joseph Winlock, invited him to become an assistant in that observatory; and so, at the age of thirty, without any previous preparation, but with an accurately trained eye and hand and experience in observation, both in his native country and in Europe, at that time by no means usual, he was enabled to realize the dream of his early life and devote himself to scientific pursuits in that department which had most strongly interested him.

His work with Professor Winlock was of brief duration, though even after leaving Cambridge he continued the association with him for some time. The attachment formed then was a strong one, and he bore in grateful remembrance the man who had given him his first opportunity to realize his early ambitions. In after years, when he came to Washington, he chose as one of his principal assistants here a son of Joseph Winlock, William Crawford Winlock, also an astronomer and for a number of years the Secretary of this Society, and to the end of his life he held these two men in affectionate memory.

In 1866 he went to the U. S. Naval Academy at Annapolis, with the title of assistant professor of mathematics, but with the understanding that his duties would lie principally in the reorganization of the small observatory whose work had been interrupted by the Civil War. There he remounted and put into service the instruments and equipped the observatory for practical and scientific work.

His stay at Annapolis, though fruitful in this regard, was a brief one, for at the end of the same year he was called to the Western University of Pennsylvania, where he became professor of astronomy and physics and director of the Allegheny observatory. This position he held for twenty years, and here it was that he carried on scientific labors of

such importance and originality as to have won the international scientific reputation and recognition which caused Professor Baird to invite him to the Smithsonian Institution as Assistant Secretary and the Regents to elect him later as its chief executive officer.

His early years at Pittsburg were spent largely in securing the proper instrumental equipment for the observatory, which upon his arrival was one only in name. It consisted of "a building in which was mounted an equatorial telescope of thirteen inches aperture, bought by the university of a local club of amateur astronomers. Besides this, there was no apparatus whatever, not even a clock, and the equatorial itself was without the necessary accessories."

This was before the period of great endowment for astronomical, or indeed other scientific research in America, and the group of men whose wealth has since enriched Pittsburg and many other places in this country and elsewhere were, with a single exception, either at the beginnings of their fortunes or without perception of the needs of science. It was imperatively necessary that money be secured for the purchase of apparatus if the Allegheny observatory were to do proper work and its director have the opportunity of pursuing his own investigations.

Many affairs of ordinary life, but more especially the growth of railroads, demanded that the common clock, upon which every dweller of a civilized land depends, should be correct, and that some plan be devised whereby other than solar time should serve over considerable areas.

Tentative efforts in this direction had been made by the Greenwich observatory, by the Naval observatory, by Harvard College at Albany, at Brussels, and at other places; but nowhere systematically, nor upon any really practical or useful plan. To the needs of the Allegheny observatory and the fruitful mind of Mr. Langley we owe the establishment of the time service, and its outgrowth, the standardization of time in the United States and in other countries, and through its financial returns the instrumental equip-

ment of Allegheny observatory was rendered possible, and likewise the great discoveries in astrophysics by its director.

At the age of thirty-five, in 1869, Mr. Langley published his first two papers, the very first being a report of two pages on the observation of the total eclipse of August 7, 1869, at Oakland, Kentucky, and the second, "a proposal * * * for regulating from this observatory the clocks of the Pennsylvania Central and other railroads associated with it."

When we recall the intolerable inconvenience which attached to the changing of time in every forty or fifty miles of travel and the empirical method by which clocks and watches were set, resulting in annoyance, confusion, delay, and disappointment, these early labors of Mr. Langley, resulting in our standard time system and in the almost universal regulation of public clocks through electrical signals from observatories, must be counted, if not an important advance in knowledge, a really great contribution to the convenience, comfort, and welfare of mankind.

While these practical efforts to secure a fund for the equipment of the observatory were maturing, Mr. Langley had the opportunity of carrying on astronomical work under other auspices. In 1869 he took charge of a Coast Survey party to observe the total eclipse of August 7 of that year, at Oakland, Kentucky, resulting in the brief paper above referred to, and in 1870 he accompanied a Government eclipse expedition to Jerez de la Frontera, which was under the general direction of Professor Joseph Winlock and included on its staff, besides Mr. Langley, Professors Young and Pickering, both of whom have since become among the foremost of American astronomers.

He had meanwhile not lost his interest in the time service, the methods of which he described in an article in the *American Journal of Science* in 1873, proposing, in addition to transmitting time to railroads, to supply it to watch-makers and jewelers and to cities in general for their public clocks.

Almost from the beginning of his astronomical work he had devoted his attention to the sun, his investigations being chiefly astrophysical in character, and among his earlier observations in this field were his sun-spot studies, carried on about 1873. From that time on until 1880 he was engaged in minute telescopic study and drawing the details of the surface of the sun, and especially of sun-spots. Photography had not begun to be used for such purposes, and his skill and accuracy in making drawings of observations of these phenomena were particularly valuable. Indeed, it is declared by astrophysicists that his sun-spot drawings made at Allegheny prior to 1875 are even yet to be regarded as the best recorded evidence of their structure. I learn from Mr. Abbot, of the Astrophysical observatory, that "Professor G. E. Hale, who has enjoyed the choicest opportunities for examining the sun, both with the forty-inch reflector of the Yerkes observatory and with the horizontal telescope on Mt. Wilson, and also during various expeditions to high mountain peaks, says that in the best views of sun-spots he has ever had, the better they were seen the more nearly have they appeared as shown in Langley's drawings." In spite of this great power of direct personal observation, he was quick to appreciate and to employ the aids which photography lends to this research, though it should be said that the standard illustration of a sun-spot which appears in most of the text books and works on astronomy of the present time is one drawn by Mr. Langley with his own hand at Allegheny in December, 1873. The following statement of his continued work in this field during his Allegheny period was prepared recently for publication in a general encyclopedia, and, having had the advantage of his own revision, it is taken as an authoritative statement of his researches:

"About 1875 he began to devote much attention to the measurement of the heat spectra of the sun and other sources of radiation. Convinced after long experience with the thermopile of the futility of attempting to discriminate the effects of narrow portions of the spectrum by means of any heat-measuring apparatus then employed, he sought to

devise something more satisfactory, and in 1879 and 1880 was successful in the invention of the bolometer. This instrument has found high favor for a wide range of experimental work, but in his hands it has been used from 1880 to the present time to open up a great new field of investigation in connection with the invisible long wave-length rays proceeding from all heated bodies, and to change many of the older ideas concerning them.

"The more important of his many researches published during this period were upon the energy spectrum of the sun, the transmission of the earth's atmosphere and the solar constant, the behavior of prisms toward long wave-length radiators, the energy-spectra of heated terrestrial bodies, and the energy spectrum of the moon, the moon's heat hitherto having been recognized with difficulty even in gross by the thermopile, but now, by the bolometer, being analyzed in minute detail in a lunar heat spectrum. More recently, a comparison of the proportions of luminous and non-luminous heat in the spectra of the sun and artificial light sources with the corresponding proportions of the light and heat in the radiations emitted by the glow-worm, gave important economical results.

"In 1881, previous observations at Allegheny having led him to believe that there was a great, and then unappreciated, selective absorption both in the sun's and in the earth's atmosphere, which rendered in the latter case Pouillet's methods inapplicable, and which, when recognized, tended to give a far larger value to the solar constant, he, with the aid of the Government, organized an expedition to the top of Mount Whitney, the loftiest mountain in southwestern California, whose abrupt precipices permitted observations to be made from two neighboring stations, yet with a distance of more than two miles of altitude between them. These observations were published by the United States Government in a volume entitled 'Professional Papers of the Signal Service, No. XV. Researches on Solar Heat and its Absorption by the Earth's Atmosphere.' Perhaps the most important result of the expedition was the entire change in the hitherto accepted value of the solar constant, while incidentally these and others carried on at Allegheny led to the displacement of the old assumption in favor of the present view, namely, that the general absorption is largest as we approach the violet end of the spectrum.

"By 1885 the solar spectrum had been followed by him to wave-lengths ten times as great as those of the visible spectrum, and radiations from terrestrial sources even further, thus overthrowing the ideas previously held of a natural limit to the infra-red wave-lengths at about 1μ . His extended bolometric researches on the heat spectrum of the moon led him to fix the maximum lunar temperature at little above 0°C . In his researches on these long wave-length spectra, Mr. Langley developed the optical possibilities and determined the constants of rock-salt, a substance already employed by Melloni, but whose range of usefulness was now very greatly extended."

But he did not confine himself during this time either to his labors in the observatory or to making their results known to scientific men through contributions to societies and journals. He had a decided opinion of the right of the world to know what scientific men were doing and a remarkable gift of presenting such knowledge to the man of average intelligence. He occasionally delivered lectures in the city of Pittsburg, which were reported for one or another of the Pittsburg papers, and wrote letters to the *Pittsburg Gazette* when any unusual astronomical phenomenon which might be of public interest presented itself. By 1875 his reputation had grown to such an extent that he was invited to lecture at Stevens Institute, and his papers, which had heretofore been published only in American journals, commenced to appear abroad in English and Italian periodicals and in the Transactions of the Academy of Sciences of the Institute of France; this, be it noted, within five years from the date of his first publication.

The trend of his mind toward the popularization of science may be judged from a paper which appeared in the *Popular Science Monthly* in 1877, entitled "The First Popular Scientific Treatise," in which he declared that "science is not for the professional student only, but that every one will take an interest in its results if they are only put before the world in the right way." The treatise was Fontenelle's "Conversations on the Plurality of Worlds," and the article, while holding strictly to its subject, showed something of

that intimate knowledge of French history to which I shall allude later on.

The question of the personal error or personal equation, which has attracted so many astronomers, also had his attention, and he described in a communication to the *American Journal of Science* in 1877, a machine whereby this personal error could be entirely eliminated.

In 1878 he took charge of a party sent out by the United States to witness the total eclipse of that year from Pike's peak, at an elevation of 14,000 feet, and, besides the scientific memoirs which resulted therefrom, and through which he was able to follow the corona to an hitherto unsuspected distance from the sun, he wrote pleasant, chatty letters describing the more personal side of the work of the party.

In the winter of 1878, during the course of a visit to Europe, he spent some time upon Mt. Etna, and made observations there which resulted in the production of scientific papers and a very interesting article entitled "Wintering on Etna," which was contributed to the *Atlantic Monthly*.

In 1881, through the generosity of the citizens of Pittsburg and with the coöperation of the United States Signal Service, he conducted an expedition to Mt. Whitney, to which reference has already been made.

Mr. Langley's general reputation shortly after this became greatly enhanced by a series of popular lectures delivered at the Lowell Institute and at the Peabody Institute at Baltimore, afterward published in the *Century Magazine*, and later still in the form of a book, which has gone through several editions, under the title of "The New Astronomy." These lectures and this work set clearly before educated people the results of his own labors and of others in that branch of astronomy which, dealing not with the questions of longitude and latitude, or the discovery of planets, asteroids, or comets, or the other problems of the older astronomers, had to do with the physics of the heavenly bodies; the study through patient observation and numerous in-

genious devices of not the mere existence of the heavenly bodies, but of their constitution.

The spirit in which this work is written can be gleaned from its very brief preface:

"I have written these pages," he says, "not for the professional reader, but with the hope of reaching a part of that educated public on whose support he is so often dependent for the means of extending the boundaries of knowledge.

"It is not generally understood that among us not only the support of the Government, but with scarcely an exception every new private benefaction, is devoted to 'the old' astronomy, which is relatively munificently endowed already; while that which I have here called 'the new,' so fruitful in results of interest and importance, struggles almost unaided.

"We are all glad to know that Urania, who was in the beginning but a poor Chaldean shepherdess, has long since become well-to-do, and dwells now in state. It is far less known than it should be that she has a younger sister now among us, bearing every mark of her celestial birth, but all unendowed and portionless. It is for the reader's interest in the latter that this book is a plea."

Of the scientific importance of this book and of the other work of Mr. Langley I am naturally dependent for my opinion upon others, but I may be permitted to say that its literary character is unsurpassed—indeed, probably unequalled—by the scientific work of any other in America, and deserves to rank among the popular scientific expositions of Darwin, Wallace, Huxley, and Tyndall.

Even prior to this Mr. Langley had been invited to lecture at the Royal Institution of Great Britain; his fame was growing and recognition was coming to him from many sources.

In the autumn of 1886 Professor Baird, after a personal conference with Mr. Langley, wrote him inquiring whether he would enter the service of the Smithsonian Institution as an Assistant Secretary in charge of Foreign and Domestic Exchanges, including the International Service, the Library, and the Publications, with the understanding that not more than half of his time should be given to the Institution and

the remainder could, as Professor Baird said, be employed in "keeping up those original researches at Allegheny University which have already secured for you so much distinction in the scientific world. The Smithsonian Institution does not desire in any way to interrupt the progress of your investigations; on the contrary, it will be most happy to facilitate them as far as lies in its power, with the hope, at some future day, of being able to give, in Washington, facilities equal, or superior, to those that you can have elsewhere." The reply of Mr. Langley, a portion of which I quote, throws an interesting side-light upon the character of the man utterly unsuspected by the world at large and known to only a few of his intimates—that is, a strong craving for real society, by which he meant intercourse with people of diverse minds and knowledge, all of whom might give him that intellectual companionship for which he hungered. Mr. Langley, on November 27, 1886, wrote from Allegheny to Professor Baird:

"I am obliged by your official letter of the 22d instant, inviting me to accept the Assistant Secretaryship of the Smithsonian Institution, and by its kind allusion to those relations of mine to physical science, which have influenced you in making the proposal.

"The opportunity for usefulness in that direction is a strong motive to me for acceptance, as I mentioned in the conference to which you refer; but I find from your letter that I did not then make my actual position plain, as I intended to do. To repeat what I then meant to say: I have no wish or ambition to tempt me from giving most of my time to physical investigation—at least now, while I enjoy exceptional facilities for this, together with a freedom which I could not expect in any subordinate position.

"My professional life here is, through the kindness of those to whom I owe more than official duty, a very pleasant one. In most respects, nor have I any occasion to leave the work of my predilection to increase my income.

"At the same time, both my professional and domestic life here are exceptionally isolated, and I have felt the need of some change which would bring with it along with society new occupation, if that could be of a kind not wholly dissociated from my accustomed pursuits."

His loneliness in the Allegheny observatory can be well imagined. Pittsburg of that day was largely engaged in adding to the wealth of the State of Pennsylvania, and indeed of the entire country, and this astronomer and physicist, student of art and literature, philosopher and dreamer, was there almost as isolated as though upon the top of a lonely peak. He told me once that he attended the meetings of the Medical Society of the City of Pittsburg in order that he might have contact with professional and scientific men, and that he walked down and toiled up Observatory hill once a week to spend Sunday evening in a room back of a drug store, in which four or five men would assemble to discuss the great things of the mind and the scientific problems of the day. It was a revelation to me, as I assume it will be to others, to learn from the letter of Mr. Langley quoted above that it was principally the desire to associate with others of his kind, and not ambition or opportunities for work, which brought him to Washington.

On January 12, 1887, Mr. Langley was appointed Assistant Secretary of the Smithsonian Institution. In August of the same year Professor Baird died, and in November Mr. Langley was elected Secretary by the Board of Regents. During his brief term as Assistant Secretary he had given much thought to the departments with which he was especially charged, the Exchange Service, the Library, and the Publications, and in these important agencies he retained a deep interest. The Exchanges he regarded as one of the principal means for carrying out the terms of Smithson's bequest "for the diffusion of knowledge among men," and to the Publications he gave an ever-increasing amount of thought, especially those which could be, to use his term, "understood of the people," developing the Smithsonian Report to such a point that today it appeals to every man of ordinary education and intelligence, and is in many places, where books and libraries are inaccessible, the sole and yet the entirely satisfactory means of keeping people abreast of the scientific advancement of the world.

The hope held out in the letter of Professor Baird, that some opportunity would be afforded here for the continuance of Mr. Langley's original researches, was made good, first through the generosity of the late Jerome H. Kidder and Alexander Graham Bell, and later through appropriations by Congress for the establishment of an Astrophysical Observatory under the direction of the Smithsonian Institution. This observatory, housed in a modest frame structure on the Smithsonian grounds and entailing an annual cost upon the Government of a very inconsiderable sum, made it possible for Mr. Langley not only to continue his researches, but to reach new and even more valuable results than had been obtained heretofore.

It is due to his initiative and energy that the people of this country have the National Zoölogical Park. He specialized in astronomy, but his interest in nature was not confined to it. He had an eager curiosity about animal life and a great love for natural scenes, and so it fell to him, the astronomer, to move successfully in the establishing of the park, which besides having high scientific possibilities for usefulness and instruction, is one of the great pleasure grounds of the people who live in this capital and to those hundreds of thousands of American citizens who annually make a pilgrimage to it.

Shortly after Mr. Langley's accession to the Secretaryship and aside from his work in the establishment of the observatory, he strongly desired to create a new activity for the Smithsonian Institution, and his first choice would have been that of extending its scope in the direction of the fine arts. But the time was not then ripe. He met opposition and foresaw insuperable difficulties, and so he reluctantly abandoned this field and put his persevering energy into the other just mentioned, the establishment of the park. But he always had the feeling that the Smithsonian Institution should act for the nation in the matter of art. He caused to be collected such art objects as belonged to it and were deposited elsewhere, and reimplanted, as it were, the idea of the fine arts in the Institution by setting aside a

room in the Smithsonian building which should be devoted to these collections. His death came at a time when the realization of this idea of his was about to have fruition.

He had for many years been in the habit of going annually to Europe, and this personal contact with the scientific men of England and of the continent and the reputation that his researches had brought to him and to the Institution, and his increased zeal in pushing forward the Exchange Service, led to a great enhancement of the international reputation of the Institution.

It was my rare good fortune to accompany Mr. Langley upon two of his European trips—first in 1894 and again in 1898. Upon the first occasion I heard him read before the Physical and Astronomical Sections of the British Association for the Advancement of Science a paper describing his work on the infra-red spectrum. The meeting was held at Oxford, and the hall, holding some 250 persons, was crowded. He spoke very simply and without notes, describing the apparatus that he had devised and brought together and the results that had been attained; and so vivid was his statement and so forceful that at the conclusion of his remarks the supposedly stolid Englishmen who composed the audience arose almost in a body and cheered. At a meeting of the Physical Section on the same occasion he discussed the future of aerial navigation. The session was held under the chairmanship of the late Lord Salisbury, Premier of Great Britain and that year President of the Association, and the discussion that followed was participated in by Lord Kelvin, Lord Rayleigh, and Sir Hiram Maxim, none of whom dissented from the views which Mr. Langley expressed.

None regretted more keenly that he that of the many great benefactions which came to American science few, if any, found their way to the funds of the Smithsonian Institution; so that relatively the activities of the Institution proper were not increased in the United States commensurate with the growth of other scientific organizations, though it should be said that after the original foundation the only important addition to the Smithsonian funds, that received

from Thomas G. Hodgkins, came during the administration of Mr. Langley.

Among his many notable addresses was that delivered in 1888, as the retiring President of the American Association for the Advancement of Science, under the title of "A History of a Doctrine," this doctrine being the views concerning radiant energy. The address is a charming one in every respect—as an historical investigation, as a summing up of results obtained, as a literary document, and as a prophecy. Some of the phrases are worthy of a great philosophic mind. "We have perhaps seen," he declared, "that the history of the progress of this department of science is little else than a chapter in that larger history of human error which is still to be written." And yet there is no pessimistic note in it, for he asks the question, "Shall we say that the knowledge of truth is not advancing?" and he replies to this query, "It is advancing, and never so fast as today; but the steps of its advance are set on past errors, and the new truths become such stepping-stones in turn."

To this same time belong other papers of great general interest, notably that on "The Observation of Sudden Phenomena," which will have a certain value even for the physiological psychologist, although designed for the astronomer primarily and containing descriptions of the personal-error machine invented by Mr. Langley; and also another paper on "The Cheapest Form of Light," this study being based upon an examination of the radiation of the fire-fly, and showing that it is possible to produce light without heat other than the light itself, and that this is actually effected now by nature's processes.

I am brought, however, to another field of scientific work in which Mr. Langley engaged, and with which his name has been identified during the past fifteen years, the subject popularly known as flying-machines, and which he denominated *aërodynamics*. Mr. Langley came before the scientific world and the public generally on this subject first in a very brief communication to the Academy of Sciences of the Institute of France, in July, 1890; second by the pub-

lication of an extended memoir in the Smithsonian Contributions to Knowledge, and third through a brief popular article on the possibility of mechanical flight, in the *Century Magazine*. I alluded above to one of the group of what would now be called "captains of industry" in Pittsburg who had sympathized with Mr. Langley and his work and had aided him in its prosecution. The name of this man, William Thaw, was commemorated in the preface to "Experiments in Aërodynamics" in the following phrase,

"If there prove to be anything of permanent value in these investigations, I desire that they may be remembered in connection with the name of the late William Thaw, whose generosity provided the principal means for them,"

though it should be said that Mr. Thaw's aid in this direction was not to be measured alone by money contribution to the experiments, for it meant much at that time that as eminently a practical man as he should have believed in what was then considered a wild idea, and have supported a scientific man in it both by money and by moral encouragement. This memoir, "Experiments in Aërodynamics," was at once republished in full in French and attracted widespread attention. Mr. Langley persevered in the study, and in 1893 he issued a second memoir, "The Internal Work of the Wind." This also appeared in English and French, and was designed to prove that aerial flight had an aid, described as the potentiality in the internal work of the wind, which would be of great moment in the practical solution of the problem.

But the painstaking experiments with the whirling table and with other forms of apparatus devised by Mr. Langley for the study of the question of aerial navigation did not content him, and although not himself a mechanical engineer, and with very inferior appliances, he took up the building of a machine, driven by a steam-engine, which he hoped would practically demonstrate the possibility of mechanical flight. There were innumerable mechanical difficulties in its construction and also in its launching, and, after failures

which would have disheartened an ordinary man, success came in the spring of 1896, when a steam-driven aërodrome constructed under Mr. Langley's direction in his own shops, engine and all, actually flew for three-quarters of a mile or more over the Potomac river. This remarkable success had world-wide recognition. It was communicated to learned bodies, was the talk of the newspapers, and in a specially written article in *McClure's Magazine* Mr. Langley himself described this trial and how he came to enter upon the subject. From his own words we learn that this was a problem with him from childhood days; that he used to lie in a New England pasture and watch the hawks soaring far up in the blue, and sailing for a long time without any motion of their wings, and this question he thought of in mature life, and set himself to inquire whether the problem of artificial flight was as hopeless and as absurd as it was thought to be. "Nature," he says, "has solved it, and why not man?" And with this question he described the experiments with the whirling table down to the actual flight. As was his wont, he discussed the attempts of those who came before him, and in simple language explained the theory upon which mechanical flight would be possible. This article, printed in 1897, closed with the following paragraph:

"I have thus far had only a purely scientific interest in the results of these labors. Perhaps if it could have been foreseen at the outset how much labor there was to be, how much of life would be given to it, and how much care, I might have hesitated to enter upon it at all. And now reward must be looked for, if reward there be, in the knowledge that I have done the best I could in a difficult task, with results which it may be hoped will be useful to others. I have brought to a close the portion of the work which seemed to be specially mine—the demonstration of the practicability of mechanical flight—and for the next stage, which is the commercial and practical development of the idea, it is probable that the world may look to others. The world, indeed, will be supine if it do not realize that a new possibility has come to it, and that the great universal highway overhead is now soon to be opened."

Immediately after the success of these experiments and shortly before the article was written, Mr. Langley passed through a most depressing period of his official and personal life, and his feelings then were no doubt reflected in its closing words. In the month of September, 1896, his two principal associates in the Smithsonian Institution, George Brown Goode, a distinguished naturalist, who was in charge of the Museum, and William Crawford Winlock, already alluded to, had prematurely passed away, and their loss was a serious blow to Mr. Langley, whose friendships were deep ones. Of both these men he wrote memoirs—in fact, of Mr. Goode two, the longer of which, presented to the National Academy of Sciences, is at once a discriminating and affectionate tribute to a great man and a dear friend.

For the next few years Mr. Langley's time was not so productive; his physical health was good, but the severe strain of his scientific labors and his personal losses tended to a depression of spirits which caused him to shrink from new work. In spite of his almost definitely announced intention no longer to carry on the work in flying-machines, he was led in 1898, through circumstances not clearly known, but which had to do to a certain extent with the Spanish-American war, to take up the building of a flying-machine large enough to carry a man, this work being undertaken under the Board of Ordnance and Fortification of the United States Army, and with an allotment made by that board for the purpose. He had meanwhile, after a little lapse of time, renewed his astrophysical work, which, through the improvement of the instruments he had invented, produced new and valuable results. The bolometer was brought to a greater degree of refinement than had ever been attained. The researches of the Astrophysical observatory had progressed to such a point as to justify the publication of a remarkable volume of *Annals*; and an expedition made by him, to observe the solar eclipse of 1900, at Wadesboro, North Carolina, was signally successful.

A half dozen or more papers illustrating the various advances made in the study of the spectrum were also issued about this time. The building of the large aërodrome and of models to aid in its construction was rapidly being pushed ahead. Since the successful flight of the first aërodrome in 1896, a further possibility of increased power with comparative lightness had come with the employment of the gas engine, and this was experimented upon with a view to determining its feasibility for the purpose.

In the midst of these labors, either of them enough to engross the thought of an ordinary man, carried along as they were in addition to the management of the Institution and its correspondence and the interviews and the appearances before committees which this work entailed—in the very midst, I say, of these labors there appeared an article, of all places, in the *Saint Nicholas* magazine, describing the Children's Room of the Smithsonian prefaced by a letter written by Mr. Langley himself, in which he appears as the attorney for the children and pleads their cause with a grown-up Museum man, and almost at the same time he wrote a curious and interesting paper describing the fire-walk ceremony in Tahiti, where Mr. Langley spent part of the summer of 1901, and where he hoped to find a miracle, but witnessed instead an interesting ceremony, which, almost to his own regret, he was able to explain by natural law.

A brief popular account of the subsequent experiments with the Langley aërodrome was published in 1905, an extended memoir on the subject being yet unpublished, though left in such shape as to render its publication certain. He describes in the briefer paper the attempt made to purchase a suitable engine or to secure its building by contract elsewhere; the acceptance of such a contract by a mechanical engineer, and the failure, after two years, to deliver the engine in accordance with agreement; the consequent necessity of building it at the Institution; the innumerable details of construction that had to be considered, and finally the trials, first of the test models, which proved successful.

Twice, on the 7th of October, 1903, and again on the 8th of December of the same year, attempts were made to launch the large machine, and in both cases, according to the observation of numerous reliable engineers, members of the Board of Ordnance, and others, it was the launching that proved a failure, and the words of Mr. Langley, in closing this statement, seemed to be justified: "Failure in the aërodrome itself," he declared, "or its engines there has been none; and it is believed that it is at the moment of success, and when the engineering problems have been solved, that a lack of means has prevented a continuance of the work."

There can be no doubt but that this failure to launch the big machine was a serious blow to Mr. Langley. Not so much the failure itself, for he was a philosopher and a scientific man, who knew that success came only after repeated defeat. Had it meant unsuccessful experiment in his laboratory or shop, it would have daunted him not in the least. It was necessary to make these experiments in the open air, before the eyes of the world, while his arrangements with the Board of Ordnance and Fortification rendered it imperative that the details of the construction should not be made public. The newspaper press of the country, misunderstanding his motives and angered possibly at the large expense connected with maintaining special correspondents at an inconvenient place on the Potomac river, united in a chorus of ridicule and attack, which in time made itself felt in the national legislature. At his years, for he was then nearly seventy, the attitude assumed by the public press broke his spirit at this the first, indeed the only, defeat in his career.

The lack of means of which he speaks was only a lack of funds from the source from which he thought he was entitled to obtain it. One or more private individuals offered him the opportunity to continue. Several years before, he had been offered a considerable sum for this work if he would but place it upon some commercial basis and take out patents

on such portions of the machinery as were patentable, in order that commercial reward might come to the persons furnishing the money, but he steadfastly refused either to secure a patent or to accept money from private persons. He declared that this work was solely in the interest of the nation, and if the nation was not prepared to support it, he was not willing to proceed with it. As far as I can learn, he never wavered in his belief that success would result from his work. Aërial navigation was, in his opinion, sure to come, and the very machine which was declared by the public press to have been wrecked beyond hope, he had repaired in absolute condition for another trial.

It is a gratification to be able to record that the last paper that he ever read was a series of resolutions adopted by the Aëro Club at New York city, expressing appreciation of his work in behalf of aërial navigation and confidence in the directions which it had taken, and any reader of the current magazines or the daily press can see for himself that, in spite of criticism and ridicule, the principles which he discovered are more and more gaining recognition. The future of aërial navigation lies not in the direction of the balloon, which is being abandoned even by its most ardent votaries, but in that of the aëroplane; and, whatever form this may take or whatever modifications may be made as the result of experiment, the laws of aërodynamics will be the laws which Mr. Langley discovered; and the aëroplane, or other form of machine heavier than the air, will be based upon the models which he made and which actually flew.

The tributes in recognition of his work are almost too numerous to recite. He received the degree of D. C. L. from Oxford, D. Sc. from Cambridge, and, among numerous others, the degree of LL. D. from the universities of Harvard, Princeton, Michigan, and Wisconsin. He was awarded the Henry Draper Medal by the National Academy of Sciences, the Rumford Medal by the Royal Society of London, and the Rumford Medal by the American Academy of Arts and Sciences, as well as the Janssen Medal from the

Institute of France, and the medal of the Astronomical Society of France. He was a foreign Member of the Royal Society of London, a Correspondent of the Institute of France, Fellow of the Royal Astronomical Society of London, Member of the Royal Institution of London, Member of the Academia dei Lincei of Rome, of the National Academy of Sciences, and of many others.

Mr. Langley, although a member of very many scientific and other societies, was not a regular attendant at any of them. He systematically avoided holding any office in any society, the only exceptions that I know of being his Presidency of the American Association for the Advancement of Science, his acceptance of the Vice-Presidency for a brief time of the American Philosophical Society and membership in the Council of the National Academy of Sciences. It was not that he failed to recognize the importance of scientific societies, but rather that he felt confident that others could attend to their management, and that his time must be guarded for his official duties and for his scientific work.

Among the many societies to which he belonged, he had an especial affection for this Philosophical Society. He was elected to membership in it in 1887, the year in which he came to Washington, and with hardly any exception read before it the scientific papers that he presented in this city. Many of you will probably remember his various papers on the infra-red spectrum, and that on mechanical flight, and I may be permitted to say in passing that no novice ever prepared a paper or lecture more carefully than he did, for while he always spoke with great directness and simplicity and clearness, apparently without effort and usually without notes, his communications were always written carefully in advance, every slide gone over, and an actual rehearsal made, and this method was one that he carried into his scientific work as well. I remember that before going on the eclipse expedition to Wadesboro there was a rehearsal almost daily for a period of nearly three months, on both his own part and that of every other person in the party,

as to the duties which each one would be expected to perform during the very few moments when the phenomenon was observable.

One would naturally suppose that what has gone before at least fully described a single man; indeed, it relates enough to fill the lifetime of two or three men; yet it by no means adequately goes to make the full picture. I have alluded above to his having been an omnivorous reader; but this is too general an expression to give any idea of the extent of his literary cultivation. He knew the German classics, but had, like many men of his generation, an especial fondness for Heinrich Heine. It is not too much to say that he knew everything good in English, though he had some special interests and had become an ardent Borrowian. He personally owned a considerable selection of the original manuscript of George Borrow, and aided in the preparation of the *Life of Borrow* by Knapp, visiting him at Oxford and furnishing suggestions and information for this interesting work. The *History of England* and, even more, the *history of France* engaged his attention. He was at one period of his life an ardent admirer of Thomas Carlyle, whose personal acquaintance he enjoyed, and it is not impossible that from him he acquired a sort of method of historical reading, for he looked to men rather than to documents of the periods as furnishing the keynotes for the progress of nations. Leonardo da Vinci, and Cromwell, and Frederick the Great, and Louis XIV., and Napoleon, and Lincoln were some of the men about whom he had read everything available to the student, and he had gone deeply into the memoirs of their respective periods, more especially, however, the French memoirs, with which he had an acquaintance that might have been envied by a professional historical student. He was especially interested in the problems of the soul, and studied the metaphysicians and the modern psychologists, and was himself associated with societies for psychical research, and personally engaged in the examination of spirit mediums, though never with satis-

faction to this keen observer. He knocked hard and loud at the door which leads to knowledge of the soul, for it seemed to have been one of the necessities of this great mind that it should attempt all the difficult problems which were offered to human observation or curiosity. He loved to talk with men possessed of positive religious views upon their own beliefs and had a deep interest in a Jesuit, or a Jew, or a Buddhist, or a Mohammedan, or indeed any man who thought he had secured the truth and knew the way of life in this world and the world to come. His paper on "The Laws of Nature" is a very significant contribution from this point of view.

He was probably less understood upon his personal side than any other. When I came here to live in 1892, I remember that Mr. Goode said to me once that Mr. Langley was a very reserved man and a very lonely one, and that though it might be difficult to gain his friendship, the effort was well worth the making. I do not know that I did make a conscious effort. In my then position as librarian, I came into official contact with him because of his very great interest in and constant demand for books of every nature. By chance I found that he was a collector of translations of the Arabian Nights, and had read all the editions in English and French available. I happened to tell him of my own interest in the subject, and the fact that as a student I had read portions of the Arabian Nights in the original. There then began a closer acquaintanceship which, I am proud to say, resulted in a friendship which has been to me one of the most profoundly valuable and touching experiences of my life. He was a very shy man, and greatly feared that he might obtrude himself upon others, or that an advance that he might make would prove unwelcome. He was also, like some other mathematicians and astronomers, at times very much abstracted and with a painfully bad memory for names, or rather an inability to associate faces and names—a difficulty which he told me had nothing to do with his scientific studies, but was inherited, and belonged to his

father, who was a merchant. This difficulty he attempted to hide as far as possible, producing upon the average man the conviction that he was dealing with a very haughty and distant individual, a deduction which was very far from the truth.

Living here without family ties, coming in his fifty-third year, almost after the period when men make close friendships, his hunger for real friendship and affection was pathetic. Most of the men with whom he came into contact were of another generation, and it was a genuine revelation to see him, as I sometimes did, with a friend of his youth, a man of his own age whom he had known for many years. He was a most rigidly truthful man—not truthful in any ordinary sense, but in that extraordinary Puritan, New England sense, which did not even permit him to subscribe himself as being “very sincerely yours” if he was not.

I have alluded above to the fact that he himself ascribed his interest in aerial navigation to a childish wonder as to how the great heavy birds which he used to watch in a New England pasture could fly and maintain themselves in the air, and in another place he has told us that his work on the sun also grew out of a childish interest in this great center of our system, upon which life on this planet depends. I think that these two ideas of his were not fancies, but that it was a fact that in his case especially the child was father to the man. One of his favorite quotations was the initial stanzas of the poem of Wordsworth:

“Who is the happy warrior? Who is he
That every man in arms should wish to be?
It is the generous spirit, who, when brought
Among the tasks of real life, hath wrought
Upon the plan that pleased his boyish thought.”

But this memorial, inadequate as it may be, must draw to a close. I have been able to faintly trace the lines of a great mind and a great soul, one that left a powerful impress upon the knowledge and thinking of the country in which he was born and the time in which he lived, and his name

and his fame are bound to be handed down through all posterity. Yet he valued these labors and the results which sprung from them but little when compared with the affection of his kin and of his friends—affections not many in number nor easily obtained, for he was, as I have said, a shy man; but he gave in full measure his confidence and his love to those whom he called friend.

A long life filled with many perplexities left his soul white. This nation and the world at large is the richer for the life of this great man.

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THE PROGRESS OF SCIENCE

AS ILLUSTRATED BY THE

DEVELOPMENT OF METEOROLOGY

ANNUAL PRESIDENTIAL ADDRESS

BY

CLEVELAND ABBE

READ BEFORE THE PHILOSOPHICAL SOCIETY OF WASHINGTON
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THE PROGRESS OF SCIENCE AS ILLUSTRATED
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ANNUAL PRESIDENTIAL ADDRESS

BY

CLEVELAND ABBE.

[Read before the Society, December 8, 1906.]

The ultimate goal of scientific research is not the collection of facts furnished by explorations and surveys, not even the exact data furnished by the most laborious measurements as in astronomy, geodesy, chemistry, and physics. Neither is it the framing of a few generalizations and inductions, such as the general idea of evolution; nor is it the establishment of some isolated fundamental laws, such as the attraction of gravitation, the conservation of energy, the mechanical equivalent of heat, the atomic weights and their periodic law. Research aims to go deeper than all this and show how these laws and phenomena result necessarily from a few simple premises—not premises in the sense of assumption, but axioms that are just as truly the basis of the physical universe as Euclid's axioms are the basis of geometry. These premises or axioms, so far as we can at present see, almost certainly belong to the realm of what we call mechanics, or the laws of force and matter; it may be the mechanics of molecules, atoms, and ions, or it may be the mechanics of solids, fluids, or gases; that is to say, it may be the mechanics of individual molecules or that of masses of molecules. Moreover, these questions of mechanics always involve some mathematical study—some graphical, numerical, geometrical, or analytical method; in every case the progress of exact science must wait on the progress of pure mathematics.

Owing to the numerous relations between the study of the atmosphere and every other branch of science, meteorology has been from time to time classed as a part of chemistry, physics, geology, and geography, but is now assuming an independence that justifies its recognition as a distinct subject; this fact requires us to explain distinctly of what meteorology consists. It is not a mere description of atmospheric phenomena, neither is it a system of maps and predictions; it is not a popular climatology, nor merely a mathematical study of the motions of the atmosphere. We must define it as embracing the broadest conceivable study of the atmosphere from any and every point of view; if we subdivide it according to the difficulty of the subject and the extent of our ignorance, beginning with the simplest and passing on to the more difficult portions, we may subdivide it into descriptive climatology, optical and acoustic phenomena, thermal or thermodynamic phenomena, hydrodynamic or mechanical phenomena. The two latter classes of phenomena constitute the subject-matter of the mechanics of the atmosphere and include all that relates to temperature, pressure, winds, cloud, fog, dew, rain, snow, hail, and the daily prediction of storms and weather. It is in the study of these phenomena that the progress of our science has been most conspicuous during the past century; the problems already partially solved involve so much of the most profound modern physics and mathematics that one cannot refuse to meteorology a notable place among the most difficult branches of science. Fundamentally, meteorology is the mechanics of the earth's atmosphere; all its other aspects are of minor importance to this and it is our progress in this line of research that should especially claim our attention.

A general historical survey of the methods by which we have arrived at the present state of our knowledge of nature will show that meteorology has passed through the various stages of development that have been common to all the sciences, and that in its present stage of vigorous growth it already stands among those that have progressed the farthest. The methods of advancing our knowledge of nature have

been the same in all ages, among all nations, and in almost all individual cases. One individual, or one nation, or one age may differ from another in its predilections for special methods, but in general we find everywhere analogous methods of thought and work, and they even succeed each other in the same order. Beginning with explorations and crude observations, man passes on to generalizations and inductions. If possible he frames speculations or working hypotheses as to the ultimate cause or the rationale of any phenomenon, and then tests his tentative deductions by experimentation until the working hypothesis has been so modified as to represent some general law. The association of several such laws leads to the building up of elaborate deductive theories, not speculations in the popular sense of the word, but well-established systems, or methods of argumentation, that represent a rational and more or less profound knowledge of nature. Such "theories" are well exemplified by Gauss's "*Theoria Motus*" or Rayleigh's "Theory of Sound."

If at some epoch a man or a nation is unable to apply any one of the above-mentioned methods of study, then the real knowledge of nature stops at that point, and man waits until the development of his powers enables him to take the next step in the line of research. But it has many times occurred that meanwhile men have spent centuries floundering about aimlessly in the bogs of ignorance, following some imaginary light like the will-o'-the-wisp. If dogmatic authority has sometimes hindered the progress of knowledge, still more has man's inherent conservatism, by reason of which he adheres to the teachings of antiquity, the practice of his parents, and the worship of his ancestors. Such conservatism may build up a family or a nation; it may insure the entailment of estates and the power of tyrants, but it is a perversion of the commandment "Honor thy father and thy mother" to doggedly insist that what is good enough for the parent is good enough for the children. The love of truth requires us not only to hold fast that which is good, but to discard that which is false. The path of progress in meteorology is strewn with the wrecks of popular errors.

NATIONAL METEOROLOGICAL ORGANIZATIONS.

Devotion to any science brings with it the formation of special organizations for its promotion, not only private academies, observatories, and universities, but national or state institutions; and meteorology has had its share of these. Of course, these organizations are not always mainly and directly for the benefit of knowledge and science, but more frequently for the material benefit of the people. In America, Henry and Maury; in England, Glaisher and Fitzroy; in France, Le Verrier; in Holland, Buys Ballot; in Austria, Fritsch, were the first to start organized national efforts to make what little we know of the atmosphere available to the practical needs of mankind. Our sister sciences, astronomy, chemistry, and biology, have given us examples of the general principle that neither the people nor their rulers will support scientific research as such, unless and except in so far as the research directly benefits or promises to benefit them. Popular appreciation of science is expressed by the question, "What good will it do us?" This is the inevitable outcome of the strenuous struggle for existence. "Knowledge is power," says one; "Knowledge is money," says another; "Knowledge is fame and position," says a third. Only the few enthusiastic individuals pursue knowledge for her own sake. The majority of the people and even of university students necessarily take the so-called "practical view" of the subject. Appropriations of public money are made in order to obtain results that are of value to the business interests of the whole nation. It remains for the administrative chiefs to decide how much of the time and money at their disposal can profitably be spent on research and how much on daily routine work; therefore the scientific and national organizations have had quite various experiences. It is within bounds to say that the meteorological offices of France and Germany began with the feeling that we know little of meteorology and must make great additions to our knowledge before attempting practical forecasts; hence in France, under Le Verrier, several years of experience were

acquired before that work began. The German office, under Dove and von Bezold, has thus far restricted itself to climatology and general theoretical studies, wisely leaving it to the new office, just now started, under Börnstein, to attempt predictions for the benefit of the public. The British office, under Fitzroy, began boldly with predictions, but was obliged to modify its plan until further study had shown how to make these more acceptable. The American office has had a happier history, for which we must thank the long-continued preparatory studies and weather-maps of Redfield, Coffin, Loomis, and Espy, which continued for fifty years from 1820 to 1870, so that we really did know something about the behavior of our special American atmosphere. But especially must we thank the preliminary daily telegraph maps of the Smithsonian, from 1854 to 1861, and the cautious policy of Prof. Joseph Henry. In 1870 Gen. Albert J. Myer, favored by an extensive system of telegraph lines, was really justified in promising to undertake storm-warnings based on the daily weather-map. Not only has our own Weather Bureau realized all that was hoped for it by its early projectors, but Prof. Willis L. Moore as Chief has now assured it a certain degree of perpetuity by adopting certain principles that insure steady progress for all future time, namely, that behind every high art there stands a higher science; that complete success in weather forecasts demands an equally complete knowledge of the sciences involved in the motions of the atmosphere; that satisfactory progress in predictions can only be based on corresponding progress in our knowledge of the physics that underlies theoretical meteorology.

But you will say that these are only the ordinary axioms of the modern civilized world. True, and it is the recognition of such axioms that marks the domination of the human intellect. The operations of the atmosphere are so obscure that multitudes doubt whether we shall ever understand it and continue to rely on the old-fashioned signs and the annual almanacs. Meanwhile meteorologists throughout the world are seeking to gain knowledge and light from every

source; everywhere kites and balloons, mountain stations and cloud observations are being utilized as means of studying the upper atmosphere, while on the other hand each national weather bureau is extending its field of observation horizontally, so as to secure a broader weather-map. The insight we get by the help of mechanics, the help we can derive from mathematical physics, the suggestions that we get from cosmical physics, the new ideas that we get from the laboratory study of chemistry, electricity, and radiation, the broadening of our field of observation by the use of wireless telegraphy, all conspire toward the better establishment of our science and consequently the perfecting of the daily predictions.

ELEMENTARY METEOROLOGY.

In every branch of human activity we begin with the simplest ideas and easiest actions, and then progress to the most complex combinations and most difficult constructions, eventually arriving at abstractions of whose essence we know nothing, but whose effects are observable and measurable. This statement applies to all branches of science, and meteorology is no exception. We begin with the direct testimony of the senses, then we recognize the abstract idea that force must pervade nature and must be the foundation of all the phenomena that we have apprehended by means of our five senses.

The simplest atmospheric phenomena were first observed, and these stimulated the earliest philosophers of classical antiquity. Until most recent times meteorology was not advanced by the work of professional meteorologists so much as by occasional contributions from those whom we ordinarily speak of as astronomers, geographers, physicists, chemists, but who in earlier times were known as philosophers.

To the astronomers we owe certain fundamental facts, namely, that the earth is a sphere, that it rotates on its axis and revolves about the sun and that its axis is inclined to the ecliptic. To establish these few simple points required two thousand years—from the days of Eratosthenes, born 276

B. C., at Alexandria, down to the time of Copernicus, who died in 1543, and of Galileo, who died in 1642.

To the students of optics we owe the explanation of the twilight, first correctly given by the Arab, Alhazen, who lived in Spain in the 11th century, but who may have drawn much of his knowledge from earlier Alexandrian Greek manuscripts that are now unknown. But even he knew nothing of the ultimate cause of the refractive power of the atmosphere; he attributed it to the transparency of the air rather than to its density; whereas Kleomedes, A. D. 50, seemed to understand that it was the density of the medium that principally determines the amount of refraction.

The rainbow and its supplementary bows and halos in general were observed more or less accurately in the earliest ages and are mentioned by Aristotle, who knew that they depended in some way upon the position of the sun. The first steps in the proper explanation of the rainbow were taken by Vitello, who began by observing carefully the rainbows formed in the spray of the waterfall at Viterbo; his work on optics was written about 1250, but first published in 1572. The complete explanation had to wait for the development of theories of the nature of light by Newton, Huyghens, Young, and Fresnel; in fact, only within the lives of the present generation have Airy, Mascart, and Pernter perfected our knowledge of halos and rainbows.

Mirage and the twinkling of the stars were also observed and fairly well described by the early writers in Greece, Italy, and Arabia. Pernter quotes authorities to show that the mirage in the desert, the "Serab," by which the traveler is deceived into thinking that he beholds a distant lake of water, is referred to in many old Turkish and Arab documents and even in the book of Isaiah. The explanation given by the Arabs was to the effect that the deceptive lake of water is due to water-vapor or fog floating over the desert; this error continued until Kepler discovered the phenomenon of total reflection of light, which had been independently discovered by Newton and given in lectures as early as 1673, though his "Optics" was not printed until 1704.

The chemical composition of the atmosphere was scarcely suspected or suggested by any of the ancient writers, and we must come down to the days of Priestley, Scheele, and Lavoisier to find anything known on the subject.

The idea that air has physical properties, such as mass or weight, and that it could offer material resistance to bodies passing through it, was often expressed, but the properties were not satisfactorily observed and measured until the days of Galileo in Italy and Stevin in Holland. Galileo, having a pump for compression, was able to show that air is a compressible gas; but having no means of pumping the air out of a receiver, he was unable to entertain the idea of a vacuum, and in fact explained the rise of water in a pump as due to the horror of a vacuum, until his pupil, Torricelli, presented the idea of the elastic pressure of the atmosphere.

Other mechanical properties of the gases of the atmosphere, such as inertia, centrifugal force, expansion with heat, density, elastic resistance, and viscosity, were entirely unknown to the ancients, and were first clearly set forth by Galileo, Torricelli, Stevin, Descartes, Huyghens, Hook, Boyle, and Sir Isaac Newton.

SOCIETIES FOR RESEARCH IN METEOROLOGY.

The association of men into academies or some equivalent organizations dates back to the remotest history. The wise men or learned priests and philosophers of Persia, Assyria, and Egypt were organized in companies connected with temples of worship and as official astrologers in connection with the astronomical observatories. The so-called library or museum at Alexandria, Egypt, founded by Ptolemy Philadelphus 250 B. C., became the center of the most famous school of science of all antiquity, and developed into a true university, which lasted until overthrown by the Arab Mohammedans. To it we owe Eratosthenes, Euclid, Diophantus, Ptolemy, Synesius, and many other mathematicians and astronomers. The observatory of Ulugh Bey and Tamerlane at Samarkand was for twenty years, 1430 to 1449, a center for the revival of Arabic science, while at the same time in

western Europe a revival of knowledge was going on that led Rudolf II to establish at Prague an academy that was distinguished by the presence of Tycho Brahe and Kepler. The modern academy of science, considered as a voluntary association of individuals for the promotion of knowledge, began with numerous establishments in Italy in the middle of the 16th century, and meteorology owes almost as much to the 300 years of activity of the *Accademia del Lincei*, founded in 1603, as it does to the ten years of the *Accademia del Cimento*.

With the invention of the thermometer by Galileo, the air pump by Otto von Guericke, and the barometer by Torricelli begins the modern period of meteorology, when accurate experiments and observations began to be possible. We thus pass from the first crude stages of observation and fancy to the days when every hypothesis was tested by observation—to the days when academies of science became prominent and when the motto of the *Accademia del Cimento* at Florence, "*Provando é reprovando*," became the watchword of science. This Academy of Experimentation devoted itself to the fundamental problems of physics; it existed only between the 19th of June, 1657, and the 14th of July, 1667; the latter is the date of dedication of its unique published volume, "*Saggi*," or Reports on the Experiments made by the Academy—a volume justly looked upon as the foundation-stone of modern experimental physics. This volume was written in the Italian, or popular, language for every one to read easily, and was intended to be the authoritative expression of the conclusions arrived at by nine of the ablest Italian thinkers. The Academy did indeed keep a diary showing everything that was said and done by each person in its daily convocations, but the "*Saggi*" contains no reference to these individuals; it makes public only that upon which all could agree. Galileo, who died 1642, January 8, (n. s.), had been dead twenty-five years, but the spirit that pervades this volume so perfectly represents that which had animated Galileo during his life that, without mentioning his name, these nine students of his reaffirmed and expanded all that he had contended for; so that it has been well said that the "*Saggi*"

reads as though the spirit of Galileo had risen from his ashes. The volume was soon translated into Latin and English, and perhaps into other languages, and exerted a profound influence upon the science of its day.

Although meteorological stations were established in 1657 in Italy, academies and societies of persons interested in the special development of meteorology began with the formation of the Meteorological Society of the Palatinate, at Mannheim, in 1780, followed by the meteorological societies of France and England about 1850; Mauritius, 1860; Austria, 1864; Italy, 1865; Scotland, 1874; Germany, 1883; New England, 1884, and Japan, 1885. Of course all the general scientific societies throughout the world have always included meteorology in some special section devoted to that and cognate subjects.

The progress made since the formation of the Mannheim Society has been entirely in the direction of the line of work that this society laid out, namely, to collect data from all parts of the world for the purpose of compiling synoptic daily weather-maps for the study of the atmosphere as a whole. It is an instructive illustration of the slowness with which mankind progresses, to recall that at the close of the work of the Mannheim Society in 1795 twelve large folio volumes of observations had been printed, and much had been written about the relations between the weather in the different parts of Europe, but, so far as we know, without the actual preparation of a single weather-map, although all its data were compiled and published for that very purpose. It was a famous physicist, Prof. H. W. Brandes, of Halle, the eminent author of a work on "The equilibrium and motion of solid and fluid bodies," who, in two dissertations, "*Beiträge*," or "Contributions to our knowledge of the weather," and "*Reptinisis*," "A physical dissertation on the sudden variations observed in atmospheric pressure," Leipsic, 1820, finally drew from these ponderous volumes the data for a series of maps showing the circulation of winds around areas of low pressure, and thus opened the way for the study of the mechanical problems involved in storms. It must be con-

fessed, however, that his work did not greatly affect the trend of thought in those days; it was too early for Germany to be able to take advantage of his teachings. Nevertheless, as the principal editor of the most famous encyclopedia of physics, he filled the first few volumes of Gehler's *Physikalisches Wörterbuch* with the most advanced knowledge of his day. The sixth volume of that work, published at Leipsic in 1837, contains an article on meteorology written by Muncke after the death of Brandes, in 1834; therein Muncke relates of himself and Brandes that in 1820 they had developed a plan (that had to be given up on account of wars between Italy and Spain) for the publication of a general European journal of meteorology, in which Muncke should devote himself to the southwestern half of Europe, but Brandes to the northeastern half. Twenty-four principal stations were selected, between Sebastopol and Lisbon, Christiania and St. Petersburg, for which they were to publish monthly tables of the individual daily readings of all meteorological elements, and to which they proposed to add every notice that could be obtained relative to the weather on those same dates for North America, East Indies, and other distant parts of the globe. Muncke remarks that this plan was rather gigantic, but it responded to the recognized fact that science was covering a broader field and that international works such as those on the measurements of degrees, observations of gravity, and voyages of discovery were already recognized as necessary. In fact, only in this way can meteorology attain to a solid basis, and it is necessary that the scientific public should be able to compare observations made at widely distant places; for the works above mentioned by Brandes had already shown that the causes upon which depend the existence of the storms in western Europe must be sought for over the Atlantic Ocean. Muncke concludes by saying: "Time will show whether the nations of Europe already so intimately related to each other will by mutual business arrangements support such a meteorological union to the furthering of the general peace of the continent." Since the days of the Palatine Society and its active secretary, Hemmer, there never has been any doubt

that a union of all the nations of the globe must be effected before we shall be able to do justice to the fundamental problems of meteorology and climatology. To this society and to Hemmer and Brandes, Germany owes her right to the claim of having taken the first steps toward the study of dynamic meteorology.

Simultaneously with Brandes but undoubtedly quite independent of his work, the leaders of American meteorology—Redfield in New York and Espy in Philadelphia—began a life-long series of studies, at first on the geometrical and afterward on the kinetic relations of winds to storms, and of storms as a whole to the adjacent atmosphere. The U. S. Army Medical Corps, the U. S. Land Office, the Regents of the University of New York and others organized systems of reports to which the Smithsonian eventually succeeded.

These organizations were primarily for the study of climate, but in 1842 Espy was appointed "Meteorologist to the U. S. Government" and with that began our national organization of coöperation with him and the Smithsonian Institution in the study of American storms. Between the theoretical cyclonologists and those who adhered more closely to the records of observations active discussion continued from 1820 until 1870, and prepared all thoughtful minds to receive the more correct views of the next generation of students based on the study of daily weather-maps.

THE CONSTITUTION AND PROPERTIES OF THE ATMOSPHERE.

To the chemists and physicists meteorologists owe a long series of researches on the constitution and properties of the atmospheric gases. This work may be said to have begun with Boyle next after the less important work of European alchemists. Galileo had shown that the air has weight. Otto von Guericke had so constructed his first air pump, (as shown by the pictures, although he himself does not say so in words,) that the heavy air should flow down and out of the vessels from which he would pump it as he pumped water. But it is to Boyle that we owe the idea that there is an elastic spring in the air, and also that the air is a complex combina-

tion of several different vapors, such as those that produce rust and those that are exhaled from the earth, the water, vegetables, and animals. Indeed, the springiness of the air excited his suspicion that there might be some vital substance diffused through the atmosphere. The experiments that he proposed to have made—that he in fact began, and that were carried out by his contemporary, John Mayow—bore on respiration, oxidation, and evaporation as the sources of new kinds of air.

Boyle was the first to suggest that the atmosphere consisted of air, properly so called, and water in a state of expansion, together with other gases that emanate from the earth and exert an injurious influence on the health. De Saussure seems to have been the first to measure the absolute quantity of aqueous vapor in a given volume of atmosphere. In 1760 Lord Cavendish showed that the vapor evaporating from water in a vacuum had a definite elastic pressure, which he measured at several different temperatures.

The temperature of the dew-point seems to have been first observed by Le Roy (1750), Dalton (1800), and Daniel (1820). The psychrometer, or wet- and dry-bulb thermometer, is generally ascribed to August (1825), but the wet bulb was used long before by Beaumé, and it was August who gave us an acceptable, rational theory of its action, while at the same time, and quite independently, Ivory in 1822, Espy in 1829, Belli in 1830, and Apjohn in 1834, introduced modifications, all of which are now combined in Ferrel's, Grassman's, and others' theories and tables for the whirled psychrometer. The relative humidity was first observed by means of a catgut hygrometer by Brander (1650), but the hair hygrometer of De Saussure (1780) and his persistent researches were the first steps in modern hygrometry.

The discovery of carbonic acid gas, or fixed air, is generally attributed to Joseph Black, of Edinburgh, who, however, had several predecessors less widely known. In 1752 he discovered that this gas is the same as choke damp, or fixed air. According to Ramsey, Black showed that the common air of the atmosphere contains a small amount of fixed air.

The next step in the separation of the gases in our atmospheric mixture was due to Rutherford, a pupil of Black, who in 1772 announced the discovery of nitrogen as the residual gas after the combustion of carbon and the absorption of the resulting fixed air. The discovery of oxygen was made independently and nearly simultaneously by Priestley and Scheele; but Priestley published his results in 1775, a year before Scheele. The recognition of oxygen as an independent gaseous element and the establishment of our modern view of the air as a simple mixture is due to Lavoisier, who published several memoirs on the subject in 1777 and 1778, while Cavendish was carrying on a parallel system of experiments in England, experiments that he began in 1777, but published only some years later.

With the discovery of nitrogen and oxygen in the atmosphere and the measurement of the exact ratios of these and various so-called impurities, the chemistry of the atmosphere halted until, in 1882, Lord Rayleigh began a research on the relative densities of hydrogen, oxygen, and other gases, concluding in 1893 by the statement that nitrogen obtained from the atmosphere was somewhat denser than nitrogen prepared from ammonia, and that the difference, though slight, was so far beyond all question that it demanded an explanation. This explanation was announced in a preliminary way in August, 1894, but was received with such incredulity that one chemist sarcastically inquired "whether the *name* of the new gas had also been discovered." But the matter was brought to a clear demonstration by diffusing the mixed gases slowly through a long train of tobacco pipes of the variety known as the "church-warden pipe," which is made of a very fine clay through which diffusion proceeds very slowly. Thus argon was discovered. Of course you will recognize the fact that this last step in the analysis of atmospheric air is not a chemical, but a physical process, illustrating the general statement that no one branch of science can endure or progress without the assistance of correlated friendly branches.

The discovery of argon paved the way to new ideas in re-

gard to the structure of the molecules of gases, ideas that threaten entirely to undermine some portions of the old kinetic theory of gases. Since the discovery of argon, chemists and physicists, working along different lines, have, as you know, devised methods of producing extremely low temperatures; so that atmospheric air, and even hydrogen, have been liquefied, and by the help of these extremely cold liquids other gaseous constituents have been discovered in the atmosphere. First, *helium* was discovered by its lines in the solar spectrum; then it was evolved from a rare mineral, named cleavite, and finally it was shown to be present in our atmosphere. Then a large mass of air was cooled down to its boiling point, and in the residue *krypton* was discovered. Finally argon was also cooled down to its boiling point and *neon* was discovered. The separation of neon from helium requires the very low temperature of boiling hydrogen, or 20.5° on the absolute scale. Although these new gases occur in our lower atmosphere only in very minute quantities, yet there is some reason to believe that eventually they will play an important part in explaining some of the electrical phenomena that are at present quite mysterious. It has been independently suggested by Huyghens and Schuster that the brilliant green line in the spectrum of krypton is probably identical with the green line in the spectrum of the terrestrial aurora borealis, showing that krypton may exist in our upper atmosphere or in the adjoining celestial space.

But we have not finished with the gases of our atmosphere, for in 1898 Madame Curie announced the isolation of two new substances, polonium and radium. These furnish an emanation, which consists of gaseous particles, among which is helium, which latter also emanates from the element thorium. Numerous other substances are now known to send out such emanations, each of which resembles some of the inert gases of the atmosphere. It seems probable that these emanations represent the degeneration of molecules of the complex elements into simpler molecules or even into elementary matter, if such there be, thus leading to a great expansion of our ideas as to molecular structure. As these

emanations are also accompanied by an ionization of one or more of the atmospheric gases, it results that the electrical properties of our atmosphere depend in some way upon them. In general, therefore, this brilliant chapter in the history of research is another illustration of the dependence of meteorology upon the progress that is being made in every other branch of science. So we have now to face a new problem in evolution. Laplace taught the evolution of the solar system from a gaseous nebula; Huxley taught the evolution of higher forms of life from elementary structures; who will now teach us the evolution of the gaseous molecules of the atmosphere and the solid elements of the earth, from the initial atoms, corpuscles, or electrons?

MECHANICS OF THE ATMOSPHERE.

Dynamic meteorology deals essentially with the study of the behavior of a true gas, dilatable with heat and compressible with pressure, but mixed with small and variable percentages of vapors that condense to liquids or solids at ordinary low temperatures. The problems of modern meteorology therefore lie in the field of *aërodynamics* and *thermodynamics*, and can only be solved in proportion as our knowledge of experimental physics shall be extended. But the proper treatment of these problems also involves the application of difficult branches of mathematics and analytic mechanics, and these subjects have not yet been developed to an extent sufficient to handle any but the simplest of the problems of nature. As we read the scientific literature of the eighteenth century we find Euler, in his "*Mechanics*" (1736), developing the fundamental formulæ for the movements of dry gases and ideal liquids, after he had proceeded as far as he could with the mechanics of rigid bodies. In his prize essay of 1746 D'Alembert developed a theory of the winds. We pass then to the great French mathematicians, Lagrange, Poisson, and Laplace, and the English mathematicians, Green and Stokes, to all of whom we owe investigations of the laws of motions of fluids under two essentially different conditions, namely, when a velocity potential

exists and when it does not exist. In 1851-55 appeared the memoir of Stokes on viscosity, and in 1857 the famous memoir of Helmholtz on vortex motions, each of which removed difficulties that had hitherto obstructed our progress. The works of Sir William Thomson, now Lord Kelvin, on thermodynamics and on circulatory motion, and the persistent researches of Bjerknes, father and son, in the application of vector analysis, have clarified our ideas and represent our present highest attainments in this branch of mechanics. Just as meteorologists have hitherto been dependent upon physicists for the apparatus with which to observe, and upon the mathematical physicists for the explanation of the optical and thermal, the acoustic and the electric phenomena of the atmosphere, so now they are coming to be more and more dependent upon the higher mathematics to resolve the analytical difficulties inherent in the complex problems of fluid motion.

It is very rarely that the meteorologist arrives at a phenomenon deductively and then examines the records of observation to see if it actually exists. Ferrel did this in a few cases; but usually we have proceeded by slow inductive methods. For instance, the Phœnician voyagers and the Greeks who penetrated into India knew of the existence of the southwest monsoon, but a complete knowledge of its origin and nature has required centuries of observation and the labors of men of great talent in mechanics. Fifty years ago it was assumed in a general way that the heated air over the interior of Asia, by expanding and overflowing, gave rise to an indraft corresponding to the southwest monsoon; but it remained for Ferrel, about 1880, to show that it was not merely a heated interior, but a heated high plateau that was necessary to produce this great current; and it was not until 1890 that Sir John Eliot showed that this monsoon current is by no means a simple disturbance of the northeast trade winds that are appropriate to the latitudes of India, but that we have to go much farther south, far across the equator, and see that the whole southeast trade-wind system of the southern Indian Ocean is perverted from its course.

Instead of rising in the torrid zone and turning back upon itself to antarctic regions, the southeast trade rushes across the equator, skirts the coasts of Africa, Arabia, India, Siam, and China, whirls around the great desert plateau of Tibet, producing the area of low pressure that is central over that region in the hot months, and finally is lost in Kamchatka. Of course this transfer of a great mass of air from the southern to the northern hemisphere during our summer must be followed eventually by the return of an equivalent mass to the southern hemisphere; but we have not yet discovered how, or when, or where that return is effected. Therein lies the secret of much of our so-called periodic or quasi-periodic and secular weather changes which depend on the internal mechanism of our atmosphere, not on solar or cosmic influences.

Finally, we now go one step further and note the fact that we may divide the surface of our globe into two hemispheres, known as the continental and the oceanic. The former has its pole on the Greenwich meridian at about 30° north, including nearly all of Europe, Asia, Africa, the Atlantic Ocean, and both the Americas, being about three-fourths land. The other has its center about 40° south, includes the greater part of the Pacific, Indian, and Antarctic regions, and is four-fifths oceanic. The sun's heat pours upon the continental hemisphere with especial fervency in May, June, and July, and upon the oceanic hemisphere in November, December, and January. The circulation of the air, both horizontal and vertical, the distribution of temperature, moisture and pressure, the resulting winds and rains over the continental hemisphere in its summer have but slight analogy with the corresponding phenomena over the oceanic hemisphere in its summer, because of the differences in the action of insolation upon land, water, and snow or ice. We are no longer justified in treating the whole atmosphere as though it were resting upon a globe of uniform surface, and subject to slight perturbations by reason of ocean-currents and small continents. We have to consider the insolation of the continental hemisphere and that of the oceanic hemi-

sphere as two disturbing forces of equal magnitude, acting on the air above these in such a way as to cause these halves of the earth's atmosphere to react on each other in a series of movements or perturbations most delightful to contemplate and most inspiring to the mathematical expert, who quickly acquires a grim determination to solve the problems that are presented. This interaction of the continental and oceanic hemispheres is responsible for the fact that what happens in India in its summer by reason of the special character of its monsoon is not only related to what happens in Africa and Siam, but even to what happens in Australia and America. A most interesting evidence of the recognition of this principle will be found in the fact that Mr. Gilbert T. Walker, the meteorologist of the Indian Service, in his annual forecast of the Indian monsoons makes a statement of the conditions affecting the monsoon rainfall in which he includes the precedent conditions over Australia, South America, and Siberia; he shows that the Asiatic and equatorial regions, taken by themselves, do not suffice to determine the future character of the monsoon.

A corresponding indication of the broadening of our field of view is found in the fact that our own Weather Bureau has lately begun to receive telegrams as to the barometric pressure prevailing in the interior of Asia, more especially in Siberia, under the conviction that the oscillations that take place in that region give some indication of what will subsequently occur in our own territory. This idea developed at once from our experience during the first year of our forecast work in 1871, and led promptly to the establishment of our "Bulletin of International Simultaneous Meteorological Observations" with its daily charts of the northern hemisphere, undertaken by General A. J. Myer in 1873, in accordance with the idea that the atmosphere must be studied as a unit. His published bulletin of international observations gives us a daily map of the whole northern hemisphere from 1875 to 1884, after which only monthly maps were published. But the daily manuscripts have continued to be compiled up to the present time, although on a somewhat different plan.

These afford a precious material for working out the relation between atmospheric movements on a large scale. The first steps in such generalizations were taken by Professor Garriott, to whom are due the conclusions given in "Weather Bureau Bulletin A, Summary of International Meteorological Observations."

Next we come to a series of charts published by Hildebrandsson about 1895, showing the simultaneous departures in pressure at many stations over the whole globe. These tables and charts show that an excess or a defect of pressure may be observed simultaneously over a very large part of the globe, perhaps one-half or even three-quarters of its surface, while in other months the conditions will be nearly reversed. Inasmuch as he took monthly averages, he was not able to show the progressive movements of these areas of high and low pressure, if, indeed, they do move, as is fair to presume and as he would probably have discovered if he could have compiled daily or pentadic instead of monthly maps.

But our observations have been largely confined to the earth's surface and to stations near sea-level; we must go higher in the atmosphere. The importance of mountain stations and of balloon work was recognized a century ago, as shown by the establishment of several mountain observatories and by the early balloon voyages of Barral and Bixio and their successors. The numerous voyages by Glaisher added greatly to our knowledge, but the systematic work with both balloons and kites since 1893 has constituted a brilliant epoch in our study of the atmosphere. Although the kite had frequently been used by Franklin and other electricians during the previous forty years, yet its use to carry thermometers to great heights dates only from Alexander Wilson, of Glasgow, 1780. A century later it was employed in England to study the upper winds by E. D. Archibald. The invention of the Hargrave or box kite and the improvements introduced in every detail by Professor Marvin, and to a less extent by others, have converted the kite into a most important meteorological apparatus. Meanwhile the use of a small balloon

carrying only self-recording instruments has been perfected by Teisserenc de Bort, of Paris, and Assmann, of Berlin, until it largely replaces the manned balloon; and as it can ascend to greater heights, it becomes our most powerful apparatus for exploring the upper atmosphere. At present the limiting height attained by kites is about 20,000 meters, and by sounding balloons, so called, 25,000 meters, although these limits are only attainable under the most favorable circumstances. The persistent use of kites at Mr. Rotch's observatory at Blue Hill and the development of the mathematical theory of the kite by Professor Marvin stimulated all European observers to undertake the same line of research, each in his own country. Mr. Rotch has also been successful in securing the means for special kite explorations over the ocean. With characteristic energy, Assmann has been able to send up either a kite or balloon, or both, every day—first at Berlin, 1899-1902, and afterward at his new observatory at Lindenberg; so that we have a continuous history of the temperature of the air above Berlin for several years, up to the highest points attained by kites and balloons. On the other hand, in the United States, the Chief of the Weather Bureau, after authorizing Professor Marvin to develop the kite, the reel, and the meteorograph, established seventeen kite stations north of a line joining Washington and Topeka, as a southern limit, with the intention of receiving the reports by telegraph and compiling a daily map of the conditions in the upper atmosphere. The work at these stations extended from April to November, 1898. The average results as to vertical gradients of temperature, humidity, and wind were compiled by Dr. Frankenfield (see *Weather Bureau Bulletin F*), but the preparation and study of a daily map of the upper atmosphere analogous to the maps that we are accustomed to use at the sea-level required a new line of thought for which the world was not quite prepared at that time. To this problem Prof. C. A. Bjerknes, of Stockholm, has paid especial attention, and his ideas have been embodied in a memoir prepared by his pupil, J. W. Sandstrom (published by the American Philosophical Society), elucidating

the first steps proper to be taken in the reduction of the observations with balloons or kites to any given level in the atmosphere. These authors make the point that as the surface of the ocean is an equipotential surface, therefore observations reduced from it upward to some other equipotential surface of gravity possess simpler relations to each other than when reduced to a uniform height above sea-level. With the help of the Carnegie Institution, Bjerknes and Sandstrom are now still further developing and improving their method with the assurance of throwing new light upon atmospheric motions.

Even when we study the motions of gases on a small scale in our laboratories, we have the greatest difficulty in understanding the processes that go on right before our eyes; still more is this the case with those that go on in the atmosphere. The smooth flow of air, like that of water, made visible by some fine floating particles, suddenly changes without apparent cause into a series of whirls and vortices, and then from whirls back to a steady, smooth flow. A vortex ring of air traverses a large room to a distance far greater than a fine straight jet can do, as though its large front surface experienced less resistance than a small jet.

Under hydrodynamics proper I may mention the works of Chree, Bigelow, Bjerknes, J. J. Thomson, Ekholm, Margules, and Rayleigh; also the discussion between Airy, Ferrel, and Kelvin as to the tides in the atmosphere, resting on the interpretation of a certain formula in the memoirs of Laplace, a subject that was finally elucidated by Dr. Ling, of Columbia University. Not only do we owe the mathematical theory of heat to Fourier and Poisson, but especially to the former a posthumous memoir on the motions of fluids in which the internal motions and the distribution of heat are mutually interdependent. This memoir is but a fragment, establishing certain differential equations, the solution of which is rarely possible when the boundary conditions are given; so that in general we must at present rely upon partial solutions and suggestions derived from experiments or observation. To Rayleigh and Stokes we owe a number of mem-

oils on fluid resistances, including the first solutions of problems involving viscosity or internal friction of fluids. To Prof. Joseph Reynolds we owe some beautiful experiments showing how the motion of a fluid changes from a laminar flow to a vortical flow whenever the excess of internal energy amounts to a very small limit, and *vice versa*.

To Willy Wien, a pupil of Helmholtz, we owe the development of problems relating to vortex and wave motions in the earth's atmosphere. To Professor Pockels, another pupil, we owe a very ingenious memoir on the influence of mountain slopes in forcing moist air to ascend and form clouds and rain. It is the presence of aqueous vapor in our air and the consequent thermodynamic complications that necessitates a combination of hydrodynamics with thermodynamics and leads to still more complex mathematical problems, whose solution is absolutely necessary if we would understand the formation of clouds and rain. In this branch of study we have an instructive memoir by Brillouin, in which he is able to explain in a general way the formation of many types of clouds or layers of clouds as due to the mixture of masses of air having different degrees of temperature and moisture. The appearances of the clouds have been most carefully observed and recorded for two centuries, but the ability to learn what they can teach us has only become possible within the past thirty years.

The first step in the application of thermodynamics to meteorology was undoubtedly taken by Espy in 1822, when he stated that the cooling due to the expansion of air ascending into regions of lower pressure caused the formation of clouds and the lower temperature of the air of the upper strata. However it was soon found that the cooling is not due to the expansion as such, but to the *work* done by expansion against atmospheric pressure. This general explanation was accepted by French physicists in 1839, but was given greater precision by Sir William Thomson in 1864, and Peslin in 1869; it was satisfactory to American, English, and French students, but seems not to have been accepted in Germany until Pro-

fessor Hann wrote an explanatory article in the *Zeitschrift* of the Austrian Meteorological Society for 1874, showing how the laws of thermodynamics apply to the atmosphere. This paper was followed by much more elaborate studies and a series of valuable publications by others, so that it is now easy to apply our knowledge of thermodynamics to the atmosphere. A most helpful memoir along this line was that by Hertz, in which he gave a very simple diagram (known everywhere as the Hertzian diagram of adiabatics for the atmosphere) for determining what the condition of moist air must be on attaining a given height in the atmosphere. Assuming that it retains its original amount of heat during the whole time, his diagram shows very clearly the results of the ascent of ordinary clear air up to a level at which cloud-formation begins; then to the level at which the precipitation is in the shape of frozen water-drops, or hail, and above that to the region in which precipitation must be in the shape of icy spiculæ, or snow. These four stages of cooling, viz., the dry stage, cloudy stage, ice stage, and snow stage, characterize nearly all the important phenomena of the weather.

In his further applications of thermodynamics Professor von Bezold has clarified our ideas by introducing a series of diagrams after the manner first taught by Clapeyron. Assuming that a unit mass of air mixed with a given quantity of moisture rises or falls adiabatically, his diagrams then show its condition at any moment by means of curves analogous to those used by the steam engineer when he wishes to ascertain the condition of the steam in his cylinder and the amount of work being done by it. Von Bezold also shows how to treat any changes in the air that are not adiabatic, although so nearly so that they can be called pseudo-adiabatic. Lately a student of von Bezold, Dr. Neuhoff, has published a modification of Hertz's diagram, together with elaborate tables, by means of which most problems in the formation of cloud, rain, and hail or snow may be very easily solved, and with as much accuracy as the present state of our knowledge allows. A still more extensive work along

this line has been published by my colleague, Professor Bigelow, in his Weather Bureau Report on International Cloud Observations. He has not only discussed all the observations of clouds made in connection with the International Program during the year 1896-7, but has added to this a memoir that is quite unique in meteorology, including a complete system of fundamental constants, formulæ, and reduction tables. I need only add that my colleague's work on the hydrodynamics and thermodynamics of terrestrial meteorology as contained in this volume will undoubtedly be recognized as perfectly sound. By collecting all important formulæ and numerical constants into one system of tables with uniform notation, he has simplified the work of young students and rendered it convenient for any one to rapidly survey the increasing literature of the subject. I especially commend his chapters 10 and 11 to experts in mathematical physics. He has arranged his numerical tables so as to make them as convenient for the solution of his problems as are the diagrams of Hertz and Neuhoff.

THE WATERSPOUT OF AUGUST, 1896.

Nothing will more brilliantly illustrate the success with which our colleague has attacked atmospheric problems than his latest memoir, which is now being published in the Monthly Weather Review on "The Waterspout of August, 1896," about which I will say a few words. The lantern slide pictures that I am about to throw upon the screen are exact reproductions, without retouching, of photographs of this spout, which occurred on Wednesday, August 19, 1896, in Vineyard Sound, Mass. It was fortunately photographed or accurately observed from at least six different points south, southwest, west, and northwest of the spout itself, the principal views being those taken at Cottage City, on Marthas Vineyard, which was about $5\frac{3}{4}$ miles southwest of the track of the spout. Fortunately a small schooner was passing along between Cottage City and the spout, and as the views always include this vessel its movement became the means of measuring the exact intervals of time. In order to derive

the best results from these photographs, Professor Bigelow personally visited the location and made sufficient measurements to enable him to convert the apparent distances given by the photographs into angles and linear distances; so that we are able to chart the position of the schooner and the waterspout from time to time during the 25 minutes embraced by the photographs. Three spouts were seen in succession, though it is probable that there was only one general whirl in the atmosphere, moving slowly southeastward while the spout-cloud appeared and disappeared. No photographs of its first appearance were obtained, but those of the second and third appearances are published as half tones in the M. W. Review for 1906 and are numbered as follows:

Second appearance:

- A, 1: 02 p. m., by Chamberlin at Cottage City.
- B, 1: 03 p. m., by Coolidge at Cottage City.
- C, 1: 08 p. m., by Hallet at Cottage City.
- D, 1: 12 p. m., by Dodge at Vineyard Haven.
- E, 1: 14 p. m., by Ward at Falmouth Heights.
- F, 1: 17 p. m., by Coolidge at Cottage City.
- G, 1: 17 p. m., by Coolidge at Cottage City.

Third appearance:

- A, 1: 20 p. m., by Chamberlin at Cottage City.
- B, 1: 24 p. m., by Chamberlin at Cottage City.
- C, 1: 27 p. m., by Coolidge at Cottage City.

By reducing the measurements made on the photographs to linear dimensions, Professor Bigelow arrives at the following figures, which will interest you, because they are certainly the first that have ever been determined accurately for any waterspout:

The diameter of the waterspout at sea-level was 240 feet; its smallest diameter midway between this and the cloud, 144 feet; at its summit, or the lower surface of the cloud, the diameter was 840 feet. The approximate length of the tube, or height from the ocean to the lower surface of the cloud, 3,600 feet. The height of the top of the cloud above its own

base was 12,400 feet, and its total height above the ocean level was 16,000 feet. The spray, or cascade of drops forming a cloudy or smoky appearance at the base of the spout, was 720 feet in diameter, and the height of the summit of this cascade was 420 feet.

As a small vessel is visible in the middle of some of these pictures, I will add that the distance from the photographic camera at Cottage City to the waterspout was 5.75 miles, but the distance to the schooner was only 2 miles; the movement of the waterspout from the northwest to the southeast was at the rate of about 1.10 miles per hour; the rate of the schooner was 1.7 miles per hour. The wind was very light at the time, as stated by several observers and as shown by the smoothness of the water. Meteorological observations are rather scanty, but from the best information at hand Professor Bigelow finds the average temperature of the air at sea-level at the place of the spout was 67.5° F., or the maximum for the day, and the thermograph for Nantucket also shows that the spout occurred at the time of maximum temperature. On the other hand, the temperature at the land stations fell rather rapidly to 56.5° at Vineyard Haven and 59.0° at Woods Hole; so that the effective temperature within the anti-cyclonic wind that prevailed around the outside of the cloud, or at a distance from the spout, was about 58° . The barometric pressure in this outside region was about 30.10 inches, but must have been about 30.05 near the waterspout. The relative humidity was low at the meteorological station. The lower strata of the atmosphere were drier than on any other day of the month, and after several trial computations Professor Bigelow accepts a relative humidity of 64 per cent as prevailing in general near the surface of the water at the time the waterspout was formed. These are the meteorological data at sea-level beneath the cloud which surmounted the waterspout. This cloud was a large cumulo-nimbus, with its flat base about 3,600 feet above sea-level, as just stated in connection with the length of the tube. With these meteorological data and the thermodynamic equations, Professor Bigelow computes the conditions in the air ascending in a rapid whirl within the center of the tube.

The preceding dimensions, computed trigonometrically, have been quoted as measured from the photographs, but the figures deduced from thermodynamic theory and Professor Bigelow's tables are as follows: The height of the base of the cloud, or the dry stage of the ascending air, should be 3,537 feet, or 63 feet less than the 3,600 measured on the photograph. The cloud stage extends thence upwards for 5,669 feet, or to a total height of 9,206 feet. Here the freezing or hail stage begins, which is a comparatively thin layer of only 243 feet, and therefore ceases at a total height of 9,449 feet. Above this all precipitation is in the shape of snow, or minute crystals, certainly not hail or frozen water-drops, and the thickness of this layer, 6,765 feet, brings us to the top of the cloud, at 16,214 feet, or about 5,000 meters above sea-level. The agreement of these thermodynamic computations with trigonometrical measurements is quite satisfactory.

Now the motion of the air depends essentially upon the change of pressure, or the gradient. An abnormal horizontal gradient will produce horizontal motion or whirlings, but a vertical gradient will produce rising or falling motion of the air. Only a short distance from the waterspout, over the island of Nantucket proper, the vertical gradient corresponded to a fall of 0.098 inch for each ascent of a hundred feet, whereas the temperature and moisture conditions over the water near the spout give a vertical gradient of 0.101 inch per hundred feet. This small difference of 0.003 inch per hundred feet corresponds to a total difference of 0.11 inch between sea-level and the cloud base 3,600 feet above. It is this difference of pressure that is the effective gradient for vertical movement, causing the air at the outer boundaries to slowly descend while the air within the tube rapidly ascends.

The main part of Professor Bigelow's memoir is devoted to explaining numerically each step in the formation of the spout and its linear and vertical motions over Vineyard Sound. From this special study he is led to investigate the whole question of the condition attending any overturning that may occur in the atmosphere. If a layer of cold air be

spread over a layer of warm air, resting quietly upon it with the help of an intervening diaphragm, and the latter be removed, we all know that the cold air must descend and the warm air rise—a process of overturning such as is occurring every day in the atmosphere. The mechanical conditions or mechanical theory of this upsetting were recently worked out by Margules, and his views, with some important modifications, are developed by Professor Bigelow in such a way that a certain conclusion is inevitably reached. This overturning takes place not merely in a small way, as in thunderstorms, but on the grandest scale in tropical hurricanes. Now the question has been discussed *pro* and *con* for a hundred years as to whence comes the energy involved in the production of the rapid rotary winds of hurricanes. Espy maintained that in thunderstorms this energy was derived from gradients due to the condensation of aqueous vapor and the evolution of heat in the clouds. I thought it due also largely to the sun's heat acting on the top of the cloud. Professor Bigelow shows that while these are true causes, yet for hurricanes they are entirely insufficient, and that the energy of these great storms is mainly derived from the gradients produced by the overturning of layers of cold air flowing from northern latitudes over the warm air that is flowing from southern latitudes; by the descent of this cold air to the ground the force of gravity gives it great velocity and momentum. In other words, we must not look upon a great storm as a symmetric cyclone with a center of warm rising air and an inflowing pericyclone of cold air, as was taught by former meteorologists, but we must face the problem of a simple overturning in the lower strata of the atmosphere below the level of the general west wind that is flowing a few miles above us. The ideal cyclone and anticyclone probably do not exist in the atmosphere. This conclusion gives precision to an idea that Ferrel fully acquiesced in, namely, that the atmosphere has no simple circulation, cyclonic or anticyclonic, but is a complex mass of interlacings of currents; so that the progress made by himself in studying ideal types must sooner or later be replaced by researches that adhere more closely to the actual phenomena of nature.

CONCLUSION.

The resolution of problems bearing on the mechanics of the earth's atmosphere is stimulating the efforts of the world's best men, and illustrates the stage to which meteorology has attained in its progress toward being an exact science. Some portions of meteorology are already as exact as our knowledge of chemistry, optics, physics, or astronomy can make them; other parts are still in an unsatisfactory condition, which, of course, is also true of every branch of knowledge. We must congratulate our colleague on the contributions that he has made along lines of research that will help the next generation of students to a more thorough knowledge of laws that will eventually become the basis of satisfactory long-range forecasts. It will always redound to the credit of the Weather Bureau to have encouraged and published such work in this difficult field.

Equally creditable to America is the conception and establishment by the Chief of the Weather Bureau of a special research observatory at Mount Weather, where for the first time in the history of meteorology the researcher has been separated from the observer; and a special institution provided for him. This seems like the realization of an idea contained in a paragraph in my address at Indianapolis in 1890: "Why found new colleges and universities to teach what is already taught elsewhere? *Exploration is the order of the day.* Give us first the means to increase knowledge, to explore nature and to bring out new truths. Let us perfect knowledge before we diffuse it among mankind, so that what we teach may with every coming year be nearer and nearer the eternal truth of God's creation."

This exhortation is as applicable today as then. Meteorology is not yet properly recognized in our colleges, nor as a postgraduate course in our universities. The science has progressed, but the universities have not kept up with it. Laboratories have been provided for chemistry, physics, psychology, wonderful observatories for astronomy and elaborate establishments for mechanical engineering, but a laboratory for the experimental study of the motions of the atmosphere has not yet been provided, although the men who could conduct it are ready and anxious to begin the good work that they see before them.

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THE EARTH
A FAILING STRUCTURE

ANNUAL PRESIDENTIAL ADDRESS

BY

JOHN F. HAYFORD

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THE EARTH, A FAILING STRUCTURE.

BY

JOHN F. HAYFORD.

(Address of the retiring President, delivered before the Society,
Saturday evening, December 7, 1907.)

The earth is a failing structure. Stresses are set up within it by many forces. It is yielding to those forces and is being deformed beyond its elastic limit. The yielding certainly occurs every year, and probably every day and every hour.

This is the proposition which I lay before you to-night. Some of the reasons for believing this proposition will be sketched briefly.

A competent structure may be defined as one which bears the stresses brought to bear upon it without permanent injury to itself. When a competent structure is unloaded, it returns to its original shape and size by virtue of its elasticity. Its deformation while loaded is fixed by the elastic constants of the material. A steel bridge under normal conditions carrying safe loads is a good illustration of a competent structure. Under every additional pound of load applied it moves to a new position. It responds to every shifting of the loads upon it. Every motion is governed by the laws of elasticity. When the loads are removed, the bridge goes back accurately to its former shape and position and is without any discoverable difference in any respect from its state before being loaded.

The characteristics of a failing structure may be best illustrated by the well-known behavior of a piece of bridge steel being tested to destruction, under tension, in a testing machine. Suppose that successively larger loads are applied and removed. In the earlier stages of the test, as long as the loads are within the elastic limit for that material, the

elongation of the piece is proportional to the load and independent of the duration of the application of the load. Under each new load the piece takes a new length promptly and maintains it unchanged as long as the load does not change. The piece returns to its original length promptly when the load is removed. The piece is a competent structure under these loads.

As soon as a load is applied which is slightly beyond the elastic limit, the behavior of the piece is very different. The elongation is no longer proportional to the load. It increases with the lapse of time under the load as long as the load is maintained. When the load is removed, the piece returns only part way to its original length. It has acquired a permanent set, a permanent deformation.

When a load much in excess of the elastic limit is applied, the stretching under the load is more rapid and a new feature of the failure begins to appear. When the piece was a competent structure, the elastic elongation was distributed throughout the piece, following a perfectly regular law. When the piece is failing under loads slightly beyond the elastic limit, the stretching is somewhat irregularly distributed through the piece. When the loads are long-continued and much beyond the elastic limit, the stretching concentrates gradually at the immediate vicinity of the weakest point of the piece. The degree of concentration of the damage increases until, just before the complete failure occurs, practically all of the distortion is taking place in the immediate vicinity of the point at which the rupture is to take place.

There are two contrasts which it is important to keep in mind between the behavior of this failing test-piece and of failing structures generally, on the one hand, and the behavior of an elastic competent structure, on the other hand.

First, the yielding of an elastic competent structure is independent of the length of time the stresses are in action. It depends solely upon the elastic constants and the inertia of the material. The bridge engineer in designing his competent structure does not consider the length of time the

loads are to be applied. On the other hand, the yielding of a failing structure increases with the lapse of time; it is very slow if the stresses are but little in excess of the elastic limit, and is faster the greater the excess of stress beyond the elastic limit. To predict the behavior of a failing structure, it is necessary to know how much the stresses exceed the elastic limit of the material and how long they continue to be beyond that point.

Second, in a competent structure the distribution of the yielding or distortion does not change with increase of load or lapse of time. With double the load applied at a given point, every part of a competent bridge yields by a double amount. On the other hand, the distribution of the permanent yielding or distortion in a failing structure is continually changing. The damage tends more and more strongly to concentrate at the weakest point or points as failure progresses.

G. H. Darwin, in his classic paper entitled "On the Stresses Caused in the Interior of the Earth by the Weight of Continents and Mountains,"* has furnished a very good way of approaching the question, "Is the earth a failing structure?" He assumes that the earth is a competent elastic structure, and upon that assumption proceeds to compute the stresses which must exist in it, due to the weight of the continents and mountains. He assumes the earth to be homogeneous in its elastic properties from surface to center. The material forming and underlying the continents is assumed to be of the same density as the material beneath the oceans. The computation is made under a sufficiently complete theory of elastic deformation to take account of the support which each portion of the material in the earth obtains from all the surrounding material. It takes into account the transmission of stress, by compression and shear, through the solid sphere in every direction from the points of application of the forces.

* Philosophical Transactions of the Royal Society of London, vol. 173, 1882, pp. 187-230.

The computation shows that continents of such dimensions and form as those which are now in existence would produce stress-differences as great as 4 tons per square inch at depths of from 600 to 1,000 miles. The stress-differences due to the weight of the continents would, according to the computation, decrease both downward and upward from a maximum value at a depth of from 600 to 1,000 miles, becoming zero at the center and approaching zero at the surface. The stress-difference at 70 miles below the surface, according to the computation, is about one ton per square inch.* The computation, based on the assumption stated, also shows that parallel mountain ranges of density 2.8, rising 13,000 feet above the intermediate valley-bottoms, would produce stress-differences of 2.6 tons per square inch.

The statement that the stress-difference is 4 tons per square inch at a given point within the earth means that at that point the compressive stress is 4 tons per square inch greater in the direction of maximum compressive stress at that point than it is at that point in the direction of minimum compressive stress. This form of statement eliminates any reference to the actual compressive stress in either direction. It is stress-difference which tends to deform and rupture the material, not hydrostatic pressure. A stress-difference of 4 tons per square inch corresponds in its effect to a compression of 4 tons per square inch applied to two faces of a cube in a testing machine while the other four faces of the cube are left free from pressure.

Darwin's conclusion, as stated in his paper, is that since such continents exist, since they do not sink down or the sea bed rise up, the materials composing the earth are strong enough to bear the computed stresses. For example, he writes: "From this discussion it appears that if the earth be solid throughout, then at a thousand miles from the surface the material must be as strong as granite." He states in a table the breaking stress-difference in granite to be from 2.45 to 4.91 tons per square inch.

* Scaled from figure 6, plate 20, of Darwin's paper.

Let us take Darwin's computations and reason from them in another way. Instead of deducing from the computations the strength of the materials composing the earth, let us take some of the known facts as to the behavior of materials under stress at the surface of the earth and endeavor to estimate whether the material composing the earth probably would bear, without permanent deformation, the stresses brought to bear upon it by the continents and mountain ranges.

When the subject is approached from this point of view, at least six reasons appear for believing that the material would undergo permanent deformation:

1. It may be doubted whether even granite will stand for geologic ages, without permanent deformation, a stress-difference as large as 4 tons per square inch. This is the breaking stress-difference of a good granite, according to the figures given by Darwin. All materials begin to suffer permanent deformation at an elastic limit considerably less than the stress-difference necessary to break them. The longer the period of application of the stresses, the farther the effective elastic limit falls below the breaking stress-difference. In the case in hand the stresses exist continuously for geologic ages. A standard authority on engineering practice gives the safe working load for granite masonry at 30 tons per square foot, or one-fifth of a ton per square inch, and states that this is about the maximum in existing structures. According to Darwin, the material within the earth must stand for geologic ages a stress-difference of 4 tons per square inch—twenty times as great as the safe load prescribed by engineers for granite—or it must fail.

2. Granite is one of the strongest of the materials found abundantly in the accessible part of the earth's crust. One may well doubt whether the heterogeneous mixture composing the crust is as strong on an average as continuous good granite.

3. Failure—that is, permanent deformation—at any point tends to concentrate further failure at and near that point. The heterogeneous mixture of material composing the earth

probably has many spots of weakness in it which will tend to cause failure long before the stronger material gives way. The stress-differences at which failure will commence therefore depend upon the strength at the exceptionally weak spots in the material rather than upon the average strength of the material.

4. The materials composing the earth's crust, except those near the surface, are subjected to high temperatures, which tend to reduce their strength. The temperature at 70 miles below the surface probably exceeds 1,900° centigrade—above the melting point of iron and many other materials, under surface conditions.

5. It is stated in Darwin's paper that one at least of the assumptions is such as probably to make the computed stresses smaller than the fact.* The speaker believes that there are other implicit assumptions, to which no attention is clearly called in the paper, which also tend to make the stresses computed by Darwin too small. For example, Darwin has dealt with a hypothetical continent represented by a regular mathematical form. His hypothetical continent resembles, as he states, an actual continent after its irregularities have been supposed smoothed out by leveling down the mountains and filling in the valleys. The stresses due to such a hypothetical smoothed-out continent are less than those due to an actual irregular continent. He has also computed the stress-differences due to ranges of mountains not much greater than some of the actual ranges which exist on the continents and has found the stress-differences nearly two-thirds as large as those due to the smoothed-out continents. In nature the two sets of stresses, those due to smoothed-out continents as a whole and those due to separate mountain ranges, exist at the same time in combination. The stresses are not in general combined by a mere algebraic addition. Nevertheless the combined stress-difference due to both at any one point is in general larger than the stress-difference due to either one alone.

* See, especially, the statement in regard to the figure of equilibrium, on page 225 of the paper.

6. As stated in Darwin's paper itself, if the earth is not solid throughout, if the outer solid crust is less than two or three hundred miles thick, the stress-difference must be much greater at some points within the crust than the value computed by him, namely, 4 tons per square inch. Now, in so far as any effect on the stresses above that depth is concerned, the part of the earth below a given depth is an exact equivalent of a liquid, if at that depth hydrostatic equilibrium exists—that is, if the pressure in all three directions is the same at each point at that depth. Now, many different men, reasoning from various points of view, have each reached the conclusion that a liquid condition, or its equivalent in the sense just indicated, is reached at a depth less than 200 miles.

For all these reasons, then, it appears probable that under the stress-differences within the earth due to the weight of the continents and mountains, the material must slowly yield, the continents slowly sink downward and the ocean bottoms rise.

One apparently invincible argument against this conclusion is thus stated in Darwin's paper: "The interior of the earth must be in a state of stress, and as the land does not sink in, nor the sea bed rise up, the materials of which the earth is made must be strong enough to bear this stress." But the premise of this argument ignores the overwhelming mass of evidence accumulated by the geologists, that large portions of the present surface of each continent have at some past time been at or below sea-level. Changes in elevation in the material composing the continents are put in evidence everywhere by geologic structure and topographic forms. The continents are in the habit of sinking and rising. We must therefore draw the conclusion, apparently contradictory to Darwin's, that the materials of which the earth is made are not strong enough to bear the stresses to which they are subjected.

This conclusion is not necessarily contradictory to Darwin's conclusion, for the reason that the stresses which cause

the continents to rise and fall may be due to some other cause than gravity.

Lest my attitude be mistaken, I hasten to add that I have the highest admiration for Darwin's paper and believe it to be a classic in its line. In it the assumptions are so clearly and carefully stated, the mathematical treatment is so broad, and the conclusions are so carefully expounded, so cautiously stated, and their relations to the possible errors in assumptions so fully set forth, that the paper will stand the test of the most searching criticism. What has happened in the line of thought followed to-night has been that Darwin's paper, without rejection of any essential part, has been supplemented by additional evidence, and additional and different conclusions have been reached. Not a single link in Darwin's chain of reasoning has been broken or damaged. By attaching the chain to more facts at one end, it has been enabled to hold up more and different conclusions at the other end.

The principal conclusion reached from Darwin's paper, as supplemented, is that if the material forming and underlying the continents is of the same density as the material beneath the oceans, the earth is probably a failing structure; that it is slowly yielding under the stresses, the continents sinking and the ocean beds rising. Please note that, on this evidence alone, the yielding is stated to be a probability, not a certainty. Because of the uncertainty, it is desirable to examine all available lines of evidence.

The theory that there exists in the earth the state of approximate equilibrium called isostasy has an important bearing upon Darwin's paper. According to the theory of isostasy, the material forming and underlying the continents is less dense than that under the oceans, whereas in Darwin's computations the densities were assumed to be the same. Pendulum observations have long been known to indicate that isostasy exists. The originator of the word "isostasy," C. E. Dutton, based his belief in isostasy on geologic evidence and pendulum observations.* Other geologists have con-

* C. E. Dutton: On Some of the Greater Problems of Geology. Bulletin of the Philosophical Society of Washington, vol. xi, pp. 51-84.

sidered that the evidence before them indicates isostatic readjustment to have occurred. I have elsewhere presented some of the geodetic evidence which proves that, at least for the United States, there is a rather close approach to the isostatic conditions.*

To the extent that it is proved that isostasy exists, that the continents are lighter and the ocean beds denser than assumed by Darwin, it is also proved that the stresses within the earth are less than his computations would make them. At first sight, apparently, it is also proved that there is decreased reason for supposing the earth to be a failing structure; but further consideration of the evidence in regard to isostasy, combined with geologic evidence, as indicated in the following paragraph, shows that, instead of concluding that the stresses are small, and that therefore the earth is not failing, we should conclude that the earth is failing under stresses even smaller than those we had believed to exist; that the earth is weaker than we had supposed.

The geologic evidence is overwhelming that within the interval of time covered by the geologic record many thousands of feet of thickness have been eroded from some parts of the earth and have been transported to and deposited upon other parts. If isostatic readjustment had not also been in progress during this interval, it would be impossible for the isostatic compensation to be so nearly complete as it is now known to be in and around the United States. By isostatic compensation is meant the compensation of the excess of matter at the surface (mountains, continents) by defect of density below, and of surface defect of matter (valleys, oceans) by excess of density below. Isostatic readjustment can only take place in two ways: It may take place by actual transfer of material beneath the surface. It may take place by change of density of matter without transfer

John F. Hayford: *The Geodetic Evidence of Isostasy*. Proceedings of the Washington Academy of Sciences, vol. VIII, 1906, pp. 25-40; and also *Geodetic Operations in the United States, 1903-06*, a Report to the Fifteenth General Conference of the International Geodetic Association, by O. H. Tittmann and John F. Hayford.

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(57)

of that particular matter—that is, by increase or decrease in volume of given masses. In either of these cases, or in any combination of them, there is permanent deformation of the material, a yielding to applied stresses, and we are led again to the idea that the earth is a failing structure.

If the earth is now and has long been a failing structure, the material accessible at its surface should bear the evidence of that fact in recognizable form, just as the twisted mass of steel on the bank of the St. Lawrence bears in itself easily recognizable evidence that a bridge has failed near Quebec. The material composing the earth's surface does bear the evidence, and the evidence has been described by thousands of men. If one consults the literature of geology, and especially that portion of it which sets forth directly the facts which have been observed, rather than the generalizations from them, he finds everywhere descriptions of faulting, of warping of strata, of folding of strata, of overthrusts, of intrusions, of vulcanism, of uplift, of subsidence. All these are evidence of past failure, of permanent non-elastic yielding to applied stresses. The only parts of the earth's outer crust where such evidence has not been found are apparently the parts of the land surface in which there has been little or no geological exploration and the ocean bottom, where the evidence would be extremely difficult to detect.

Moreover, the geologic evidence shows that these yieldings have occurred at various epochs throughout the whole interval of time covered by the geologic record written in the rocks, and that the yielding, at least in some cases, has been gradual rather than sudden. In whatever area on any continent a geologist works he finds this evidence, and, so far as I am able to judge, it appears that the more a given area is studied, the more conclusive is the evidence that permanent deformation under stress has there taken place. Just as the geologist who studies cubic miles of the earth finds abundant evidence of past failure, so also does the geologist who studies cubic millimeters of rock under the microscope find similar evidence in the structure of the rock.

One of the characteristics of a failing structure is that where failure begins, there later damage tends to concentrate. The point of initial failure in the Quebec bridge was first located mainly by noting that two of the main members of the bridge had suffered much more distortion than the others. So, if the earth is a failing structure, we should expect to find indications that progressive and concentrated failure has occurred at some places rather than that the failure has been uniform in degree in all parts. Such indications exist and have frequently been recognized and commented upon. It is recognized by geologists generally that there are regions of excessive deformation, as if some incipient weakness had concentrated failure there, and that there are other regions in which the deformations have been moderate in amount, as if those were regions of more than average strength in a failing structure. Where mountain-building commences, there it tends to continue for a long time, though not, perhaps, for an indefinite time. Ocean bottoms and great plains apparently tend to remain ocean bottoms and plains.

Some of the reasons have been put before you for believing the earth to be a failing structure, for believing that it is yielding in a non-elastic manner to the stresses produced by gravity and by other forces which are applied continuously for long periods. Is there a contradiction between this conception of the earth as a failing structure and the known behavior of the earth in earthquakes and under the action of tidal forces?

So far as I have been able to trace the connection, there is no contradiction whatever.

The motions at the earth's surface due to earthquakes, which are detected by seismographs, are vibrations of small amplitude, having periods of a few seconds, as a rule. The fact that these vibrations persist for a considerable time, for minutes or hours, and that they not infrequently travel all the way around the earth and at least part of a second trip, show that, under these particular applied forces, the earth

acts as a highly elastic body. The observed rate of propagation of the wave, combined with the known density of the material through which the wave is propagated, enables one to compute the rigidity. Such computations have been made. But the rigidity is simply the relation between the applied force and the distortion produced. It is the stiffness. Such computations furnish no information in regard to strength. As soon as one considers the stresses involved, at points remote from the source of an earthquake, it is evident that they are insignificant as compared with the elastic limit of the material.

I conceive the earth to be a failing structure, slowly yielding to the forces acting upon it. I conceive that, at the source of an earthquake, stresses having accumulated to the breaking limit, a fracture occurs, and there is sudden movement, causing, as a rule, various other fractures at near points. From all these points of fracture elastic vibrations start out in every direction, and progress until they die out on account of internal friction; and this may not be until the vibrations have traversed the whole earth. This conception of elastic vibrations traversing a failing earth, which is at the same time slowly yielding in a non-elastic manner to other stresses, contains an apparent contradiction which is, however, not real.

It is not uncommon nor mysterious for a mass of material to respond with elastic vibrations to suddenly applied forces, while at the same instant it is yielding in a non-elastic manner to forces applied steadily without change of direction. A large mass of cold pitch will resist a light blow of a hammer, while at the same instant it is slowly yielding and flowing under the comparatively small stresses due to its own weight.

In the deep copper mines of the northern peninsula of Michigan the behavior of the whole earth, with respect to earthquakes and stresses due to other causes, is well illustrated on a small scale. At certain times during each day blasts are set off in the solid rock at various places in each mine. Each battery of blasts is a miniature earthquake. In

that particular spot, the earthquake center, the rock is fractured within a space limited by a radius of a few feet. Within a large space, limited by a radius of a few hundred feet, elastic vibrations are set up in the solid rock which are sufficiently violent to be perceptible to the touch and to the hearing. Within this larger space no fracture of the rock occurs. Feebler vibrations doubtless extend out for miles from the point of fracture, just as vibrations extend over the whole earth from an earthquake center. Now it also happens that in the lower levels of these deep mines, at a mile below the surface of the earth, the solid rock is slowly yielding, in a non-elastic manner, under the influence of the great weight above it, so that the larger openings are gradually closing up. This is so clearly recognized and progresses so rapidly that it is proposed as routine practice,* at the deep levels in these mines, to take out the ore at the distant end of each drift first. The miners will then work back slowly toward the shaft from which the drift is entered, while the spaces in which they have recently labored gradually close up behind them. The gradual collapse known to be in progress occurs apparently by imperceptible flow and by minor fracturing, but not, as a rule, by catastrophes which close up any opening suddenly. In this respect it is an epitome of what is taking place every year in the failing earth as it yields under such stresses, due to gravity and other causes, as are applied for long periods without changes of sign. The solid rock near each large opening in the mine responds to every blast in the mine by elastic vibrations; and yet, at the same instant, that same rock is yielding in a non-elastic manner to the stresses due to gravity.

It is not necessary to go a mile below the surface of the earth to find non-elastic slow yielding. It is commonly known to miners working in mines of ordinary depth.

Each earthquake is evidence that at a particular point or

* F. W. McNair: Some Problems Connected with Deep Mining in the Lake Superior Copper District. *Science*, January 4, 1907, pp. 13-18.

region at that particular time the earth is failing. As the sensitive seismographs now in operation show that there are hundreds of earthquakes each year, the sum total of such failure is not small. But I repeat that the observations of earthquakes furnish little or no evidence of the absolute strength of the earth; for in or near each earthquake center, where the test of strength occurs, no accurate observations are ordinarily secured. Outside the region in which the test of strength takes place, seismographs secure a record of the elastic vibrations, and thus secure evidence as to the earth's rigidity—its stiffness, but not its strength. If the elastic vibrations were recorded by a seismograph so close to an earthquake center that the stresses during the vibration approached the breaking limit, then some information as to the strength might be secured; but such a fortunately located seismograph is so badly shaken that it furnishes no readable record.

If the earth is a failing structure; if throughout considerable portions of its mass the stress-differences are so great that the material is yielding in a non-elastic manner, or is about to yield in that way, if the sudden motions at earthquake centers are examples of such yielding, and if such stress-differences are due to the weight of the continents, or to other causes which act for geological ages with but little change, then we should find that earthquakes are more frequent at times when stresses due to other than these secular causes are greatest, and less frequent when the temporary stresses are least. According to the conception I have put before you, the earth is like a revolver with a hair-trigger. Just as a slight touch on the hair-trigger will discharge the revolver, so very slight unusual stresses in the earth should tend to produce earthquakes. This is apparently the fact. Omori, the Japanese earthquake expert, states that several such relations exist.* At times of high barometric pressure earthquakes of land origin are unusually numerous. Earth-

* Publications of the Astronomical Society of the Pacific, vol. xviii, No. 100, August 10, 1906, pp. 235-241.

quakes are more frequent when the earth and moon are in certain relative positions than at other times, the unusual stresses being produced by the weight of the water concerned in tidal movements. More earthquakes occur in years when the range of variation of latitude is great than in years when the range is small, and Omori states that "all of the destructive earthquakes of recent years in Japan occurred exactly or very nearly when the latitude [at Tokyo] was at a maximum or minimum." The variation of the latitude from its mean value—that is, the variation of the pole of the earth from its mean position—causes small unusual stresses in the earth.

Such relations of earthquakes to other phenomena indicate that the earth is now a failing structure.

The action of the earth under the tide-producing forces furnishes no information in regard to the strength of the earth. The stresses thus produced in the earth are much too small to serve as a test of strength. In Darwin's investigation, from which he concluded that the stress-differences in the earth due to the weight of the continents might be as great as 4 tons per square inch, he also concluded that the greatest stress-difference produced in the earth by the moon's tide-generating influence was less than 2 pounds per square inch (128 grams per square centimeter).^{*} Such feeble stresses, far within the elastic limit, produce measurable elastic deformation, and thus furnish a test of rigidity, of stiffness, but not of strength.

It is important for various reasons to know whether the earth is a failing structure and to what extent it is failing.

Curiously enough, in so far as our personal safety is concerned, the weaker the structure the safer we are. On a very weak earth continuously failing the changes in elevation are so slow that we have plenty of time, as a rule, to move back when the sea encroaches upon the land, and we

^{*} See page 205 of Darwin's paper.

are seldom shaken so severely by earthquakes as to disturb our comfort. If the earth were stronger the stresses would accumulate for a longer time and to a greater intensity before failure took place; but when failure took place it would be much more apt to bring disaster, by violent shocks and large changes of elevation; therefore the more firmly you are convinced that the earth is so weak as to be failing continuously, the safer you should feel.

It is important to know whether the earth is a failing structure or not, because such knowledge is necessary to a true understanding of a many of the phenomena observed upon the earth. For example, it is a standard idea of the text-books, and indeed of many of the specialists in earth-sciences, that the flattening of the earth at the poles, its ellipticity, indicates that it was formerly a liquid earth, or is now a liquid earth with a thin crust. If the solid earth is a failing structure in the sense and to the extent that I believe it to be, said standard idea is nonsense.

To those who are endeavoring to make progress in the study of the earth, it is especially important to know the true answer to the question, "Is the earth a failing structure?"

If the answer is "No;" if the earth is, in the main, a competent elastic structure, subject only to minor and local failure, then future progress will be made largely by the elasticians, by those who study the earth as an elastic structure, as did Darwin, for example, in the investigation frequently referred to to-night.

On the other hand, if the true answer is "Yes;" if the earth as a whole is a failing structure, failing frequently or continuously, failing in many parts; if failure is the rule rather than the exception, then future progress in studying the earth is reasonably certain to be made mainly by investigating the manner of failure. These investigations cannot be effectively made by mathematics applied to the laws of elasticity. The elasticians may furnish rough guides, may indicate limits approached but not reached; but the real progress will be made by those who study the non-elastic

behavior of the materials composing the earth; by those who study the behavior of the material at the regions of failure; who study its mode of fracture and of flow as it fails; who study the relations between stresses and microscopic structure; between pressure and density; between stresses and chemical condition; between stresses and solution and re-deposition; between pressure and the change of state, as between the gaseous, liquid, and solid conditions; between possible stresses in the material and its temperature; between stresses and the growth of crystals.

Briefly summarized, some of the reasons for believing the earth to be a failing structure are as follows: If the earth is considered a competent elastic structure, it is found that the computed stresses produced by the weight of the continents and mountains are so great that the existence of these stresses is doubtful, and therefore the assumption of competency doubtful. Pendulum observations and geodetic observations of deflections of the vertical indicate that the distribution of density in the earth near the surface now corresponds rather closely to the condition called isostasy, which is the condition of approximate equilibrium toward which the heterogeneous earth would tend if it were a failing structure. The approach to perfect isostasy is apparently so close as to indicate that isostatic readjustment by failure under stress has been going on in recent geologic time, during which there have been large transfers of material on the surface by erosion and deposition. The geologists find in the rocks abundant and widespread evidence of failure under stress, and evidence of a tendency to concentrated deformation in certain regions, which is a general characteristic of failing structures, in contrast with the uniform and regular deformation which is a characteristic of competent structures. Earthquakes, apparently resulting from local failure in each case, are widespread and numerous. In the laws of frequency of earthquakes in a given region there is found evidence that the earth is, as a rule, under stress so intense as to approach the breaking limit.

Finally, it must be borne in mind continuously by those studying the earth that there is no fixed and necessary relation between rigidity (or stiffness) and strength, and that a given portion of material may behave in an elastic manner under one set of suddenly applied stresses, while at the same instant it is yielding slowly in a non-elastic manner under other stresses acting for long periods without change of direction.

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ON THE TEMPERATURE AND STRUCTURE OF THE SUN

BY

O. LUMMER

ADDRESS DELIVERED BY INVITATION BEFORE THE PHILOSOPHICAL SOCIETY
OF WASHINGTON, MARCH 21, 1907

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MAY 25 1908

ON THE TEMPERATURE AND STRUCTURE OF THE SUN.

BY

O. LUMMER.

Address delivered by invitation before the Philosophical Society of Washington, March 21, 1907.

Twenty years ago, while traveling on the Mediterranean to the Holy Land, I met a number of American clergymen, sent there by their churches, and I had occasion to listen to their sermons. They told their hearers of God's almighty power and goodness, and illustrated by accounts of what they had seen in traveling around the world. The beautiful ocean, the brilliant blue sky, the lovely Italian country, all nature and its creatures, must impress upon us the idea of God, who governs all things. If such a superficial observation of nature brings us nearer to an eternal being, how much more must the scientific man, who examines more closely into the play of forces in God's universe, really appreciate their power and scope. Indeed, only the learned man of natural science reaches a conception of God in all its fullness.

The modern school of naturalists, and especially physicists, has little of the exaltation of our HAECKEL. Our knowledge of things has been reduced to the standpoint of DU BOIS-REYMOND's "*Ignoramus et Ignorabimus*." Indeed, when we ask ourselves what the aim of natural science is, we must answer: It is to study, not what is beyond the conceivable, but the laws of nature, to classify objects and phenomena outside ourselves, and to find the relations between effects and causes. Thus only do we become masters of the natural forces and turn them to the profit and benefit of mankind. Thus is blind idolatry replaced by adoration of the general

laws of nature, and of the Creator of all these wonders. To bring new phenomena and their producing forces within our established knowledge we have really not to *explain*, but to *formulate* them. Do you really *know* that our earth revolves about the sun, and *why* they attract each other according to Newton's law? We can never really know this, and indeed we shall consider ourselves fortunate when we shall have found that the Newtonian attraction requires time to influence an attracted body. We cannot know much about the real causes of natural forces; nevertheless we already know much about their laws.

According to Herman von HELMHOLTZ, our human natures must have believed *a priori* that the processes in nature are regularly and logically ordered and connected to each other by laws, or we would never have had the courage to undertake tedious and difficult research work. At the end of a long period of experimental work we feel confident in the relations which have been found to connect the observed phenomena. In order to reach the high standpoint to which natural science has brought us, we have had to abandon the old fashion of thinking out at the writing desk the mysteries of the world. In spite of Goethe's sentence, "Und was sie Dir nicht offenbaren will, Das zwingst Du ihr nicht ab mit Hebeln und mit Schrauben," we have succeeded in finding the natural laws only by experimental study, by asking Nature questions and by compelling her to answer. Not long ago this manual labor "was completely scorned." It will interest you to know that Helmholtz's father, a Potsdam theologian, ridiculed his famous son, when he became a student of natural science, for entering so unworthy a profession. It was not until the son was twenty-seven years old and a full professor of anatomy at Königsberg, and earning more money than the old man, that father Helmholtz became reconciled to the research work formerly so heartily despised by him.

At the present time salaries are paid by governments for research, and if they are not sufficient to make us wealthy, we get enough money to live upon, and, what is still more

important, large, well-equipped laboratories for research work. And, furthermore, the position of a professor in a German university is so highly regarded in my country, is so independent of presidents and governments, that I should stand in awe of them, almost as of divinity, did I not myself belong to the same class. In my country, since the time of Luther, all the original thinkers have been university professors, who, in addition to their duties, have done research work without asking or receiving payment for such work. Let me tell you a little incident about HELMHOLTZ's reply when he was fêted by the scientific representatives of all nations upon his seventieth anniversary. When HELMHOLTZ was praised as a benefactor of mankind, he answered in his characteristically modest manner, "Well, let me tell you, I did not start out to make the world happy; what I did was only to solve the questions which arose in my own mind." He wondered at the glare of the cat's eyes in the darkness and studied this curious phenomenon until he learned how the light enters the eyes and is returned from the retina in such a way that it may be observed. Thus he became the inventor of the famous ophthalmoscope, which in the hands of clever physiologists and physicists became the instrument with which to examine our eyes and to protect them from many serious ailments.

Thus all our technical industries profit by the original research of investigators who are not inspired by dreams of wealth, but who *must* think and work in order to satisfy their own inquiring natures. These men cannot be bought; they must be born; and you will pardon me when I say that many such men are born in Germany, where idealism has been inherited for several centuries.

I need not tell you that all our electrical progress and industries follow from the observation of the quivering of a frog's leg, which GALVANI or, as it is said, LADY GALVANI made when turning the electric machine, which at that time was quite a new discovery.

Let me give you another and more modern example of the technical or practical results which follow from pure

scientific problems and their solutions. I mean the radiation-laws of the black body, which were introduced into physics as a definition by KIRCHHOFF, the discoverer and founder of spectrum analysis. We will lose no time, for time is valuable this evening, in discussing the dispute which arose among the scientists of different nations over the priority of the discovery of Kirchhoff's famous law of absorption and emission of light.

Even before Kirchhoff's time the law,

$$\frac{E}{A} = \text{constant}$$

was known, where E denotes the emissive power of any body and A its absorptive power. But this law has no real importance if we do not refer E and A , as KIRCHHOFF did, to the same temperature and spectral region. It is only in the form,

$$\left[\frac{E}{A} = \text{const} \right]_{\lambda, T}$$

that this law has a scientific value. In this form it says that a body at a certain temperature, T , and in a certain spectral region, λ , emits those rays, which it absorbs, at the same temperature and of like wave length. The theoretical development of this law became the basis upon which spectrum analysis was built. Starting from this law, KIRCHHOFF and BUNSEN looked for the coincidence of dark Fraunhofer lines in the solar spectrum with bright spectrum lines emitted by terrestrial substances. For the same reason KIRCHHOFF constructed his famous spectroscope of high resolving power, with which, after much tedious labor in mapping many thousand spectrum lines, he could assert that iron and other terrestrial substances are found on our sun. The probability of a coincidence of sixty lines being due to chance is $(\frac{1}{2})^{60}$. Furthermore, since KIRCHHOFF succeeded in inverting the sodium lines by sending through the sodium flame the light of an arc lamp, he assumed the sun to be a highly heated body surrounded by radiating gases and vapors.

We must read the papers of that time in order to recall the revolution which was caused by Kirchhoff's theory and the astonishment of the old school, which believed the sun to be a cold body surrounded by a hot ocean. The excitement and the discussions caused by Kirchhoff's new solar theory were as great as when ROENTGEN published his discovery of the X-rays or when Sir William HERSCHEL discovered new kinds of solar rays. "Where we have no light at all, we have the most heat," Sir William HERSCHEL wrote in his first publication, in 1800. Since that time the radiation of the sun has been studied continuously down to the present time, culminating in the famous researches of Mr. LANGLEY, whose acquaintance I was proud to make when I was in your country over thirteen years ago.

Of so great importance were the conclusions from Kirchhoff's law with respect to spectrum analysis that the real weakness and strength of the law of absorption and emission were overlooked.

Let me first call your attention to the real importance of Kirchhoff's law before discussing the limitations of his conclusions. In my opinion, Sir William THOMSON and so many other scientists need not have questioned the priority of Kirchhoff's law, even in its precise form, for they all overlooked the importance to this law of the definition of the black body. You all know that KIRCHHOFF formulated his law as follows:

$$\left[\frac{E}{A} = S \right]_{\lambda, T}$$

where S is the radiation of the "black" body, E and A the power of emission and absorption of any other body, all symbols referring to the same temperature and the same spectral region. This means that the ratio of the emission to the absorption of any arbitrary body is equal to the radiation of the black body, and when we know the radiation laws of the black body, we only need to determine the absorptive power of another body at every temperature and for every wave-length in order to know its power of emission—

that is, its law of radiation. Thus the black body is connected to the whole material world.

KIRCHHOFF appreciated the great importance of knowing the radiation laws of the black body. In his celebrated paper in 1860 he wrote: "The radiant energy of the black body is a function only of the wave-length and the temperature, to know which is of highest importance. The experimental research is attended with very great difficulties, but it is to be hoped that some one will in the future succeed in determining this function, which surely is of a simple form, for all the functions known up to the present time, which do not depend at all on special properties of the bodies, are *simple*."

Knowing these laws as we now do, the law of KIRCHHOFF has grown from a qualitative to a quantitative one. In order to know the radiation laws of the black body or, as we will call it, *black radiation*, it was necessary to realize this curious body, which does not exist ready-made. In fact, until one was constructed by W. WIEN and myself in 1895, it was not possible to reach the high standpoint of today. According to its definition, the black body must absorb every radiation and not allow any wave to be reflected or transmitted. Lamp black and platinum black come near to this definition, but they cannot be heated to high temperatures. But, since reflection depends on the refractive index of the two media, it cannot be zero for all the incident frequencies or waves; thus a really *black surface cannot exist*.

In considering how to make a reflecting substance—for example, bright platinum—free from reflection, and thereby make it a black radiator (platinum in thick plates transmits no radiation), W. WIEN and I came to the conclusion "*that a cavity of uniform temperature must emit, through a small aperture in the wall, black radiation corresponding to the temperature of the cavity*." By such an arrangement the black body is realized with an almost theoretical perfection. But it is indeed remarkable that this realization failed to be attained for nearly forty years, when we read in Kirchhoff's original memoir the following statement: "If a space is en-

closed by bodies of the same temperature and if these transmit no rays, then every wave within the cavity is, in its quality and intensity, equivalent to radiation from an absolutely black body of the same temperature; it is independent of the nature and size of the bodies and is dependent only on the temperature." It is only a short step from Kirchhoff's conclusion to our realization, although it was overlooked even by the creator of this theory. If you will pardon my using a primitive example, the short step was: "Punch a hole in the closed cavity as COLUMBUS did with the egg!" I will omit the description of the experimental steps in the practical realization of cavities of uniform temperature within an interval of temperatures from -180° C.

up to 2300° C. and will only give the results relating to the radiation laws. From the distribution of energy of black radiation in the spectrum, represented in Fig. 1, according to the observations of LUMMER and PRINGSHEIM (where the abscissa represent the wave-lengths λ in $\mu = 0.001$ mm. and the ordinates the corresponding emission at the different absolute temperatures T), we can deduce the following laws:

$$\int_0^{\infty} S_{\lambda} d\lambda = aT^4 \quad (1)$$

$$\lambda_{\max} T = b \quad (2)$$

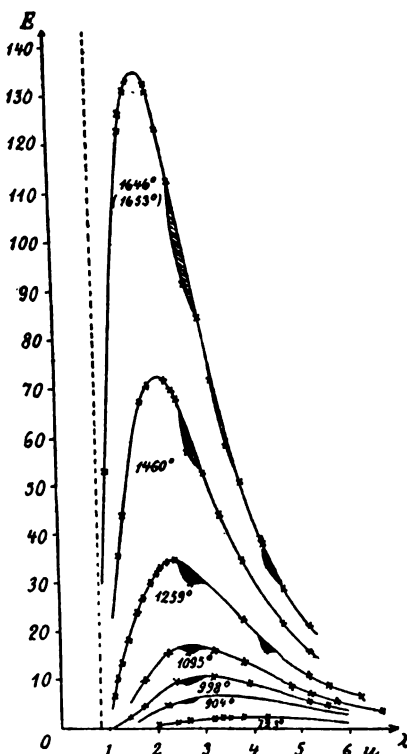


Fig. 1.

$$S_{\lambda} T^{-4} = \text{constant} \quad (3)$$

$$S_{\lambda} = \frac{c^2 h}{\lambda^5} \frac{1}{e^{\frac{ch}{k\lambda T}} - 1} \quad (4)$$

where S_{λ} denotes the energy of the black radiation with respect to the spectral region $d\lambda$ between wave lengths λ and $\lambda + d\lambda$, and T the absolute temperature, and where the constants are:

$$a = 7.061 \times 10^{-15} \text{ erg / cm}^3, \text{ degree.}$$

$$b = 0.294 \text{ cm, degree.}$$

$$h = 6.548 \times 10^{-27} \text{ erg, sec.}$$

$$k = 1.346 \times 10^{-16} \text{ erg / degree.}$$

$$c = 3 \times 10^{10} \text{ cm / sec.}$$

From these quantities we can, as a first application, calculate according to PLANCK's ingenious theory which leads to the spectral-equation (4), the number of atoms of an ideal gas in 1 cubic centimeter at 0° Centigrade and atmospheric pressure, $N = 2.76 \times 10^{19}$.

Since one mole carries the electric charge of 96,580 coulombs or $96,580 \times 3 \times 10^9$ electrostatic units, and since one mole has N molecules, the electric charge on one single molecule is 4.5×10^{-10} electrostatic units. Using also the constant of gravity

$$f = 6.69 \times 10^{-8} \text{ cm}^2 / \text{gr. sec}^2$$

and the velocity of light

$$c = 3 \times 10^{10} \text{ cm / sec}$$

we obtain according to PLANCK the "natural units" of length, mass, time, and temperature by equating the above constants to unity:

$$\sqrt{\frac{fh}{c^3}} = 4.03 \times 10^{-28} \text{ cm.}$$

$$\sqrt{\frac{ch}{f}} = 5.42 \times 10^{-5} \text{ gr.}$$

$$\sqrt{\frac{fh}{c^5}} = 1.34 \times 10^{-48} \text{ sec.}$$

$$\frac{1}{k} \sqrt{\frac{c^4 h}{f}} = 3.63 \times 10^{31} \text{ }^\circ\text{C.}$$

These natural units depend only on the radiation of the black body, the force of gravitation, and the velocity of light.

According to the measurements of LUMMER and PRINGSHEIM, the radiation laws of the black body are exact up to 2300° C., and can be considered to be true laws of nature. We are therefore justified in basing upon these laws our scale of temperature. Each of these laws can serve as a foundation upon which to establish a method of measuring temperatures.

Let me tell you in a few words about the experimental verification of the first law,

$$\int_0^\infty S_\lambda d\lambda = aT^4.$$

In the following table our results are reproduced:

No.	I. Type of black body.	II. Abs. temp. observed.	III. Reduced deflection.	IV. C × 10 ¹⁰ .	V. Abs. temp. calculated.	VI. T. obs.— T. calc.
1	Steam boiler. . .	373,1	156	127	374,6	— 1,5°
2	Fused salt bath.	492,5	638	124	492,0	+ 0,5
3	"	723,0	3 320	124,8	724,3	— 1,3
4	"	745	3 810	126,6	749,1	— 4,1
5	Fire clay furnace	810	5 150	121,6	806,5	+ 3,5
6	"	868	6 910	123,3	867,1	+ 0,9
7	"	1378	44 700	124,2	1379	— 1
8	"	1470	57 400	123,1	1468	+ 2
9	"	1497	60 600	120,9	1488	+ 9
10	"	1535	67 800	122,3	1531	+ 4
			Mean. . . .	123,8		

In the second vertical column are given the absolute temperatures, T , based on the standard work of HOLBORN and DAY, who compared the thermoelement of LE CHATELIER with the nitrogen thermometer. The third column contains the radiant energy measured with a bolometer and reduced to the same scale at the corresponding temperatures, T . It is equal to zero, if the temperature of the black body equals that of the bolometer, which was $17^{\circ}\text{C.} = 290^{\circ}\text{Abs.}$ Therefore, if the STEFAN-BOLTZMANN law is to be satisfied, we must have:

$$[A = C(T^4 - 290^4).]$$

where A is the reduced deflection of the galvanometer-bolometer at the absolute temperature, T , of the black body. The constant C multiplied by 10^{10} is given in the fourth column, from which we see how exactly the law is satisfied. We get a better criterion of this agreement by calculating the temperature, T , using for C the mean value 123.8, and putting it into the equation

$$A = 123.8 \times 10^{10} (T^4 - 290^4).$$

Thus we get the calculated values of T given in the fifth column. Our observations show how closely the black radiation varies as the fourth power of the absolute temperature.

Supposing this law to be correct with reference to the black body and supposing our sources to give true black radiation, we can correct the earlier scale of temperature established by WIEN and HOLBORN. According to this scale, based on the air-thermometer, our results showed a certain regular deviation from the Stefan-Boltzmann law. We were confident enough, or, may I better say, daring enough, to assume the temperature scale to be incorrect. And so it was!

In an excellent research, HOLBORN and DAY repeated the very difficult comparison of the thermoelement and the gas thermometer, using nitrogen instead of air, and their new scale agreed with our observations of the fourth-power

law more exactly than was expected, considering the enormous difficulty of measuring high temperatures and black radiation.

Thus we were enabled to conclude from our observations that the hypothesis is correct on which BOLTZMANN based his theoretical development of the fundamental law of black radiation. Boltzmann's deduction is based upon the second law of thermodynamics and on Maxwell's theory, according to which the radiation pressure measured in dynes is equal to the energy in ergs which is contained in the unit of volume of the medium through which the radiation passes. There can be no doubt of the applicability of the second law of thermodynamics to pure temperature radiation, where we have to deal with disordered radiation, containing waves of all lengths, phases, and amplitudes, such as the radiation emitted by a black body. Later we will show that Kirchhoff's law is not at all applicable to luminescent bodies, and therefore that Kirchhoff's conclusions with reference to the sun's temperature and to spectrum analysis in general are not correct.

Since the second law of thermodynamics is applicable to black radiation, the astonishing agreement between theory and experiment can be considered as a proof that the other hypothesis used by BOLTZMANN is also correct. In other words, that by our experiments not only the existence of the ether pressure, but also the magnitude of this pressure is established. The pressure produced by the sun's radiation upon our earth is represented by a weight of only a milligram per square meter. Therefore we admire still more the excellent work of LEBEDEV and of NICHOLS and HULL, who experimentally demonstrated the pressure of a light source in a very convincing and striking manner.

Here we have the first result of practical importance which includes within its scope even the stars. Indeed, from the existence of the radiation pressure we must conclude that two hot bodies like the sun and our earth exert on each other not only an attractive force, but also a *repulsive* one. This result introduces into the processes of nature quite a new factor and

explains phenomena which up to the present time have been mysterious "question-marks" in the sky. I mean the comets, with their tails extending away from the sun, and growing, near the sun, at the rate of thousands of kilometers a second.

One of the most fruitful applications of the black radiation laws is the determination of temperatures of light sources like our stars and our sun. Let us suppose for the moment that all our light sources emit black radiation. We then get their temperatures at once by the equation

$$T = \frac{2940}{\lambda_{\max}} \text{ degrees absolute}$$

when we know the wave lengths $\lambda_{\max}$, where the maxima of their energy curves are situated. According to the famous experiments of LANGLEY, we have $\lambda_{\max} = 0.5 \mu$, and thus for the temperature of the sun

$$T_{\text{sun}} = \frac{2940}{0.5} = 5880 \text{ degrees absolute}$$

The Hungarian astrophysicist, Baron von HARKÁNY, has, by the same method, calculated from earlier observations the temperatures of several fixed stars and found some of these temperatures to be higher than that of our sun.

This determination of the sun's temperature is correct only if the sun's radiation is really black radiation. Who knows anything about the nature of the sun's radiation? If it does not emit black radiation, perhaps it may emit only platinum radiation, which is one of the least black of all.

PRINGSHEIM and I have determined as accurately as possible the radiation laws of bright platinum in order to throw some light on these problems, the solution of which are also of importance in artificial illumination. Our experiments have led to the following laws:

$$\int_0^{\infty} E_{\lambda} d\lambda = \text{const. } T^5$$

$$E_{\max} T^{-6} = \text{const.}$$

$$\lambda_{\max} T = 2630.$$

How near we came to the truth you may learn from the fact that a recent theory of ASCHKINASS led to the value $\lambda_{max}T = 2666$.

ASCHKINASS starts out from the spectral equation of the black body and with the aid of Kirchhoff's law replaces the absorptive power A by R , the power of reflection. Since metals transmit no radiation we get:

$$E = AS = (1 - R) S.$$

Thus, when we measure R in terms of $S = 100$, the radiation of any body is

$$E = \frac{100 - R}{100} \lambda^{-5} \frac{1}{e^{\frac{c}{\lambda T}} - 1}$$

For long waves and metals of high conductivity, HAGEN and RUBENS established the relation

$$100 - R = \frac{36.5}{\sqrt{\lambda}} \sqrt{w},$$

where w denotes the specific resistance of the metal measured in ohms and λ the wave length of the radiation from the metal. Therefore, if we suppose the radiation made up of long waves, for which alone this equation holds, we have

$$E = C \times 0.365 \sqrt{w} \lambda^{-5.5} \frac{1}{e^{\frac{c}{\lambda T}} - 1}$$

and considering only such low temperatures that E_{max} is also within the scope of the Hagen and Rubens equation, we finally get from the condition $\frac{\delta E}{\delta \lambda} = 0$

$$\frac{c}{\lambda_{max}T} = 5.477$$

or, when we use $c = 14600$, as found for a black body,

$$\lambda_{max}T = \frac{14600}{5.477} = 2666.$$

Using the well-known relation $w = w_0 \frac{T}{273}$, where w is the resistance of a pure metal at T° absolute and w_0 the resistance at 0° C, we get:

$$E_{max} = C \times 1.334 \times 10^{-23} \sqrt{w_0} T^6$$

$$\int_0^\infty E_\lambda d\lambda = C \times 4.936 \times 10^{-20} \sqrt{w_0} T^5$$

Is it not astonishing that, in spite of all the limiting hypotheses introduced into this calculation, we get exactly our observed results? From this agreement we may conclude that these limiting hypotheses are valid within an even greater range than had been supposed, and, on the other hand, that LUMMER and PRINGSHEIM have observed the true radiation laws of pure bright platinum. This was not easy to do, as you will see, when I tell you that PASCHEN found the total radiation of pure platinum to be proportional to the 5.432 power of the absolute temperature, believing that this power was correct to the third decimal place.

Using the observed relation $\lambda_{max} T = 2630$ and assuming the solar radiation to be platinum radiation, the sun's temperature would be

$$T_{sun} = \frac{2630}{0.5} = 5260^\circ \text{ Abs.}$$

Thus we can fix the temperature of all radiating substances whose radiation is intermediate between that of a black body and platinum within the two limits:

$$T_{max} = \frac{2940}{\lambda_{max}}$$

$$T_{min} = \frac{2630}{\lambda_{max}}$$

With the aid of this hypothesis LUMMER and PRINGSHEIM determined the temperature of the following light sources:

	λ_m .	$T_{max.}$	$T_{min.}$
Arc lamp.....	0,7 μ	4200° Abs.	3750° Abs.
Nernst lamp.....	1,2	2450	2200
Welsbach mantle.....	1,2	2450	2200
Incandescent lamp.....	1,4	2100	1875
Candle.....	1,5	1960	1750
Gas flame.....	1,55	1900	1700

Furthermore, having thus determined the temperature of a light source, we can use the spectral equation for calculating the distribution of energy of black radiation at that temperature and compare it with the distribution curve observed. In Figure 2 the dotted line represents the calculated distribution and the heavy line the observed curve. By such

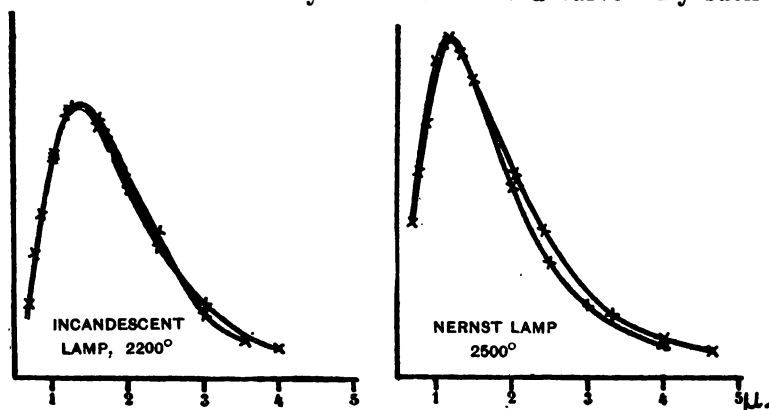


Fig. 2.

experiments we see that all the substances radiating in our practical sources of light are, like the sun, far from being "ideal" light radiators, which I defined to be *black bodies with respect to the visible spectrum and perfect mirrors with respect to the invisible spectrum*. Is it possible that in nature such ideal light radiators may exist? I do not know, but if they do exist it is possible to raise the economy of light production to 10 or 100 times that attained at present from the incandescent lamp, candle, etc. The economy might thus be increased by as much as the total radiant energy in the invisible spectrum surpasses that within the visible spec-

trum reaching from wave-length 0.4μ up to 0.8μ . (See Fig. 1.) The small progress made by the Nernst lamp, the osmium, and similar new incandescent lamps is, in my opinion, due entirely to the raising of the temperature. Indeed, the light intensity is increased in the proportion

$$1:2^3:3^3 = 1:4000:600000$$

when we raise the temperature in the ratio 1:2:3. Thus, our sun emits per unit of surface 600,000 times more light energy than the filament of an Edison lamp, and increasing the temperature of this lamp by only 100° *doubles* the brilliancy of its filament. A single Edison lamp heated up to 3000° , at which temperature the filament is destroyed, lights a large lecture room like an arc light of 2000 candle power.

Only when we are able to excite directly the electrons within the atom (or, to use my analogy, the bees in a beehive or the bells in a belfry), that is, when we have to deal with *luminescence*, do we get a relatively great economy in light production, much greater than can be obtained by heating a body like carbon or platinum or the black body. Radiating gases and vapors, as, for example, in Geissler tubes or in the mercury arc lamp of ARONS (introduced into the trade by HEWITT), are, therefore, much more economical light radiators than any of the other practical light sources. These are still sources of heat more than of light, and from a theoretical standpoint are not very far in advance of the lamps of the ancients.

When KIRCHHOFF, starting out from his law and his famous experiments on the reversal of spectrum lines, drew the conclusion that the sun's central parts must have a very high temperature, he was right, but he was not right in drawing this conclusion from his hypotheses. Indeed only when the reversing body is a black body and when the radiation from the flame, the spectrum lines of which become inverted, is due to the temperature only, can we conclude that the reversing light source has a higher temperature than the flame. But this is not the case, since PRINGSHEIM showed that the incandescence of gases, in so

far as they emit a line spectrum, is caused by luminescence and not by high temperature only.

Since we do not know whether the sun's radiation is that of a black body or that of bright platinum, it is fortunate that we have a better method of estimating the temperature of the sun from its total radiation. According to the STEFAN-BOLTZMANN law, we have

$$S_1 = a T_1^4$$

$$S_2 = a T_2^4,$$

where S_1 and S_2 are the total energies radiated by a black body at the absolute temperatures T_1 and T_2 . Thus we have

$$S_1 - S_2 = a (T_1^4 - T_2^4)$$

and according to KURLBAUM's observations,

$$S_{373} - S_{273} = 0.01763 \frac{\text{gr. cal.}}{\text{cm.}^2 \text{ sec.}}$$

Thus we get

$$a = \frac{0.01763}{373^4 - 273^4} = 1.28 \times 10^{-12} \frac{\text{gr. cal.}}{\text{cm.}^2 \text{ sec.}}$$

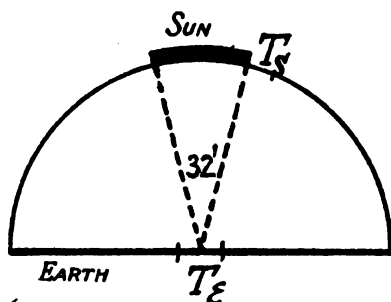


Fig. 3.

Let us now assume the sun to be a black body. A black hemisphere at the temperature T_s radiates per second to a unit of surface of our earth at the temperature T_e , the energy

$$S_s - S_e = 1.28 \times 10^{-12} (T_s^4 - T_e^4) \frac{\text{gr. cal.}}{\text{cm.}^2 \text{ sec.}}$$

Let us put $T_s = 20^\circ + 273^\circ = 293^\circ$ absolute. For a solid angle of $32'$ and for an interval of time of 60 seconds we have, therefore,

$$S_s - S_e = 1.3 \times 10^{-12} \times 60 \times \sin^2 16' \times [T_s^4 - 293^4],$$

which energy is equal to the solar constant $2.5 \frac{\text{gr. cals.}}{\text{cm.}^2 \text{ min.}}$ according to ABBOT. Thus we get,

$$T_s = 6250^\circ \text{ Abs.}$$

If the sun's radiation is not black, it can emit the same energy only at a higher temperature. Let us assume the absorptive power, a , of the sun to be not unity, but less than that of a black body—for example, 0.4, 0.1, or 0.01; then, since T varies as $1/\sqrt{a}$ we get

a .	Abs. temp.
1.0	7000°
0.4	9000°
0.1	13000°
0.01	24000°

You will observe that the temperature determined by the total energy does not depend very much on the hypothesis that the radiating body is black.

Thus we can assume with reasonable certainty that the sun's temperature is about 7000°, and we need make no distinction between the various temperature scales. You will recall that in earlier times the temperature of our sun was estimated to be 1500° by the application of incorrect radiation laws, or to be ten million degrees by the application of mechanical laws. Such a temperature (7000°) surpasses the critical temperature of all our terrestrial substances.

Above their critical temperatures, substances cannot exist at the same time in two phases, as liquids and as vapors. The highest possible pressure may compress a gas above its critical temperature to about the density of a liquid; but there never occurs a rapid transition between the gaseous and liquid phase—that is, there is never a liquid surface;

therefore a sharp limit between a liquid and a gaseous mass on our sun is physically not possible. This difficulty, overlooked in earlier times in our physics of the sun, was removed by SCHMIDT, a high school teacher in Germany. According to Schmidt's theory, the sharp limitation between the white incandescent photosphere and the gaseous chromosphere is an optical illusion, produced by the sun's being a radiating gaseous sphere, the density of which decreases gradually from the center to the outside.

It is evident that gases at a high density, such as must exist in the central mass of our sun, emit a continuous spectrum, as observed by our dispersion apparatus, like solid or liquid substances of a similar density. Corresponding to the

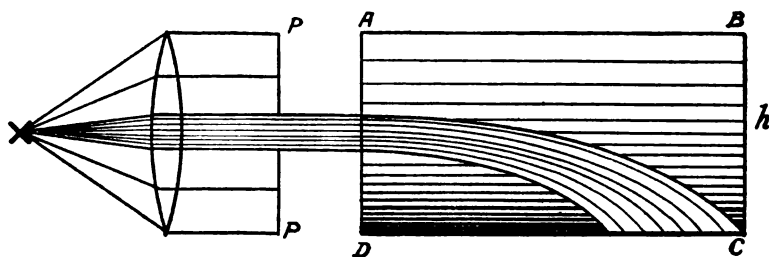


Fig. 4.

decrease of density from the interior to the exterior parts of the sun, the refractive index also decreases gradually, so that light rays emitted by the central parts follow not straight, but curved lines.

When a parallel beam falls on a plane surface, AD , of a medium, $ABCD$, the refractive index of which decreases gradually from below upward, the rays are deflected as shown by the accompanying figure. The deviation is greatest at normal incidence. This deviation accounts for the fact that we see the sun before its rays can reach our eyes directly. The bending of light rays is also the cause of the phenomenon of "Fata Morgana," observed in the desert.

In 1860 our mathematical authority, KUMMER, concluded from his theory of curved light rays that on Neptune we could see our own backs without using any mirror, since

the radius of curvature of the refracted rays can become less than the radius of Neptune. On the earth the least possible radius of curvature of refracted rays, ρ , Fig. 5, is always greater than the radius, r , of the earth, and thus an observer can never see objects which are situated on the opposite side of the earth.

In the opinion of SCHMIDT, our sun, as a gaseous sphere, belongs to the Neptune type. Since its radius is 108 times

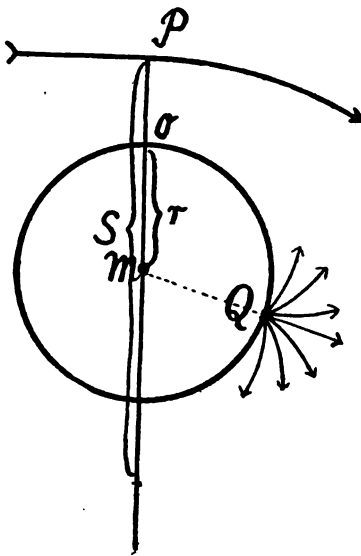


Fig. 5.

as great as the radius of the earth, all rays of every wavelength emitted by the central mass of gas return again to this central mass (like the ray aob in Fig. 6), as do also the rays which come from parts near the center; therefore from these parts no ray leaves the gaseous sphere in a direction perpendicular to the diameter, MAS , of the sun, and no such ray reaches the observer, O , on the earth. Only one sphere, which we will call the *critical sphere*, is characterized by the radius of curvature, ρ , of tangential rays equal to the radius, MA ,

of the sphere itself. A ray once exactly perpendicular to the sun's diameter at this sphere would never leave it, but would run forever around its periphery. This critical sphere, therefore, is the exterior limit of those parts of the sun from which rays, LAO , emitted by the central mass of gas can leave in a nearly tangential direction and reach the observer, O , on the earth. Rays emitted from more distant zones in a tangential direction, as RSO , have their origin *not* in the white incandescent central regions, but in exterior zones, filled with rarefied gases (the chromosphere) *emitting line spectra*. The "critical" zone

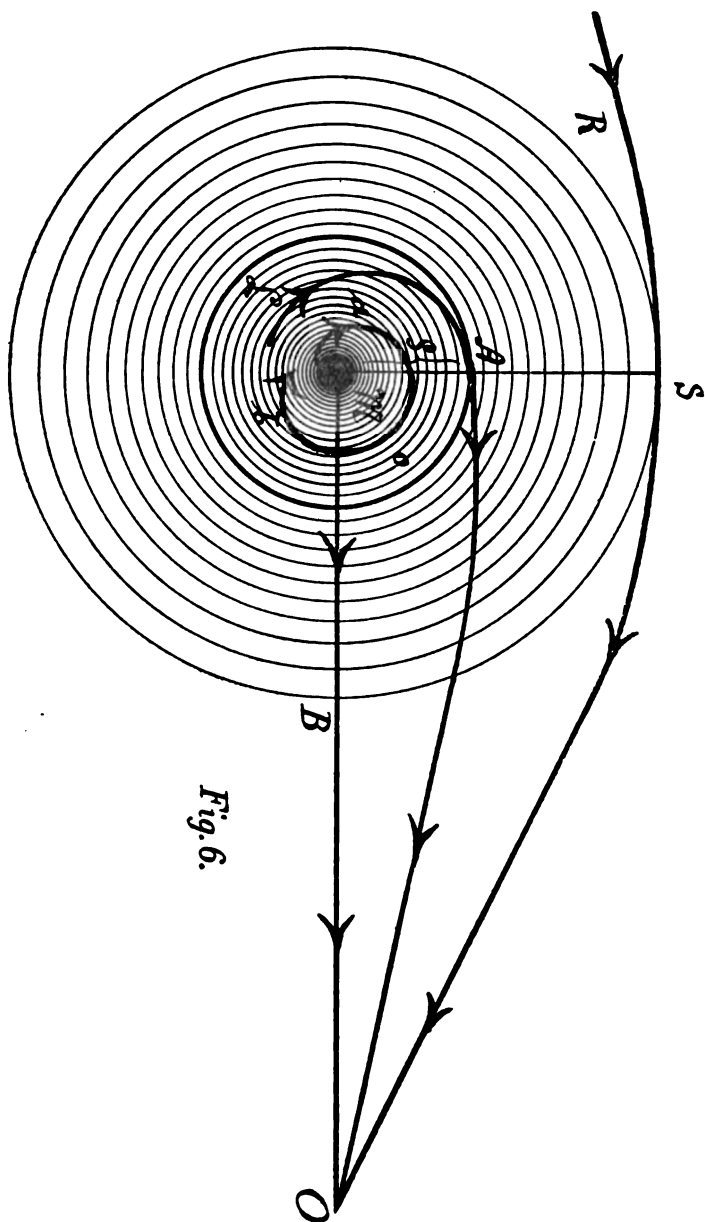


Fig. 6.

therefore defines a *sharp limit* between the region from which only light of the white incandescent center of the sun and those regions from which only the light of gases emitting line spectra reaches our eyes at O (Fig. 6). The sharp edge of our sun is, according to SCHMIDT, no real limit between liquid and gaseous parts of the sun, but an *optical illusion* produced by the refraction of light on the sun. How large the radius, ρ , of the critical sphere is and many other similar questions are problems to be solved by future speculations and observations.

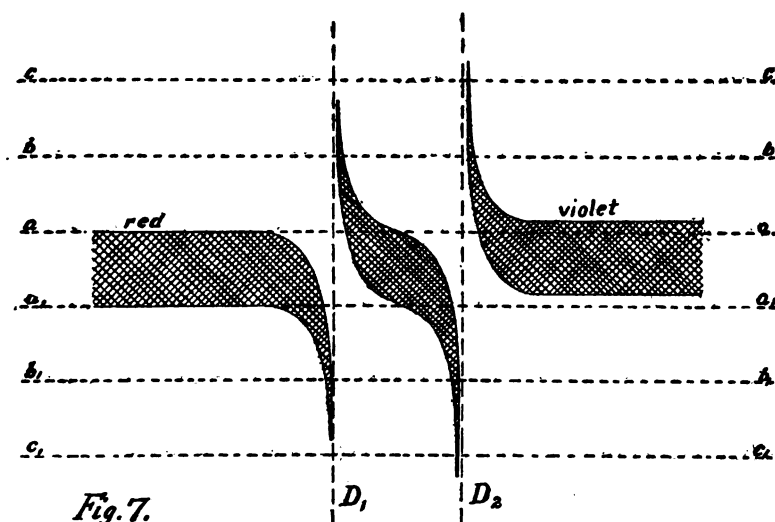
In Schmidt's theory the dispersion phenomenon plays only a secondary rôle, since in general the small dispersion of gases causes the radius, ρ , of the critical sphere to appear imperceptibly different for different colors. But a gas emitting spectrum lines has, as you know, with reference to the emitted light waves, the power of *anomalous dispersion*. Starting from this, H. W. JULIUS, the well-known Dutch physicist, has succeeded in giving a simple explanation of spectral phenomena with respect to the sun hitherto not plausibly explained.

Anomalous dispersion was observed first for sodium vapor by LE ROUX and A. KUNDT by using the well-known method of crossed spectra. With respect to the theory of Julius, it was of great importance to establish anomalous dispersion also for substances, such as calcium and strontium, the vapors in which are predominant in the chromosphere. WILSON, at Potsdam, contradicted Julius' hypothesis, inasmuch as he could not observe the anomalous dispersion of these substances.

PRINGSHEIM and I succeeded by using as a vapor prism the flame of an arc lamp colored by vapors of *Ca*, *Sr*, *Na*, etc., and transmitting through the prismatic arc flame the strong light of an ordinary arc lamp. The transmitted light falls upon a spectroscope of high resolving power, whose slit and prism edges are perpendicular to the edge of the vapor prism (cut out from the colored arc flame), as required by Kundt's method of crossed spectra. Using sodium vapor, we observe in the spectroscope the interesting phenomena rep-

resented in Fig. 7. The yellow light, the wave-length of which corresponds to those of the *D* lines, is *not absorbed*, but *deflected* in consequence of the anomalous dispersion. Becquerel's and Julius' observations had previously shown that the two *D* lines are anomalous to a different extent. From Wood's experiments we now know that the anomalous absorption of the *D* lines is larger than that of other sodium lines.

The same effect that is produced by the artificially arranged prismatic mass of a gas is produced by its gradually



variable density, as is the case in the spherical gaseous mass of the sun. In this case the observer with his spectroscope is always at a great distance from the "vapor prism," which disperses anomalously, so that he cannot observe simultaneously the whole phenomena. (In the laboratory we get the same effect by shortening and lengthening the slit of the spectroscope.)

Let us first imagine* an observer at such a great distance

* In this discussion, we follow closely the treatment given by E. Pringsheim in his publication "Ueber Brechung und Dispersion des Lichtes auf der Sonne." Archiv. für Math. und Physik, 4, 316-330, 1903.

from the sodium prism that he can observe only a small part of the whole dispersion phenomenon. Observing only the part between the horizontal lines, aa and a_1a_1 , he sees a spectrum which shows broad, dark D lines. He will naturally, therefore, suppose that these dark bands are broadened absorption lines, or, what is the same thing, that the missing light has been absorbed by sodium vapor of great density. We now know that this supposition is wrong, since the missing light has not been absorbed, but deflected by anomalous refraction to other parts (between lines aa and cc or a_1a_1 and c_1c_1). Thus: *Anomalous dispersion can simulate a broadening of absorption lines.*

In the spectrum of sun spots such a broadening of the narrowest Fraunhofer lines is observed, and it was supposed that those substances in the sun spots with broadened lines have a particularly great density. I feel certain that the missing light is not absorbed, but refracted by anomalous dispersion. To prove this, we need only suppose that in the sun spots the densities of the gaseous masses vary to a high degree from point to point. That such should be the case is not astonishing when, with FAYE or SPÖRER, we regard the sun spots as "whirlpools" in the solar atmosphere.

Observing only the parts between bb and cc (or b_1b_1 and c_1c_1), the spectroscopist sees only two narrow *bright* lines, so near to the position of the dark D lines in the solar spectrum that the difference would be difficult to measure. Probably he will imagine that he is observing the bright sodium lines emitted by sodium vapor. Thus: *Anomalous dispersion can simulate also a brilliant line spectrum.*

I feel certain that the observed line spectra of the so-called "flash spectrum" of the chromosphere and of the prominences are not due to the emission of the vapors themselves—for example, sodium, calcium, strontium, etc.—but are derived from the regions nearer to the center of the sun, that has been deflected by anomalous dispersion so as to be seen outside the sharp edge of the sun.

Let us suppose that ZZ (Fig. 8) limits the critical sphere. A ray leaving tangentially from A passes along the slightly

curved path AO and reaches the observer, O , on the earth, while a ray leaving the critical zone at B goes in the direction BO' . Let us further suppose that there exists above A a cloudy mass of gas, as, for example, sodium vapor of unequal density. Surely, then, light-waves of a frequency close to the D lines suffer a much greater deflection by anomalous dispersion than all other waves. Thus rays of frequency N_D may leave the white beam BO' at h (above A), where the gaseous cloud is situated, and pass along hO , parallel to AO , to the observer at O . Thus the observer at O will see above the sharply defined ball of the sun a region which shows a bright line spectrum. This spectrum of the chromosphere contains, as you know, more bright lines the nearer its origin is to the edge of the sun. According to the old theory, the

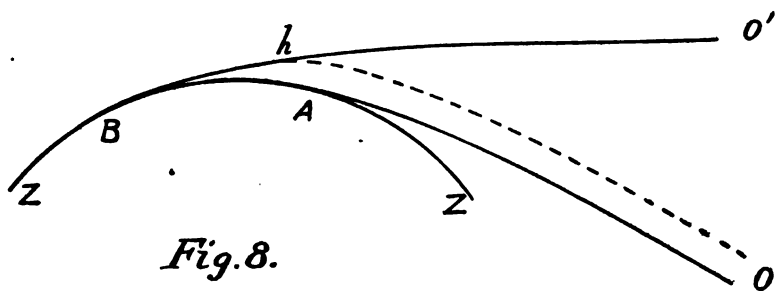


Fig. 8.

different gases must be considered arranged regularly one above the other. Now it is a great advantage that we can consider the gaseous substance a homogenous mixture, a product of complete diffusion. According to Julius' theory, the different lines of the spectrum of the chromosphere are to be observed at different distances from the sun's edge, since they suffer different anomalous dispersions when traversing mixed substances of unequal absolute densities.

According to Fig. 7, these apparent spectrum lines must become broader the nearer the point of observation approaches the edge of the sun. This is what is actually observed. Furthermore, these bright lines generally correspond to Fraunhofer lines, but show different characteristics with reference to relative intensity and position in the spectrum, which is in accord with Julius' theory.

Its most striking consequence was that the bright lines of the chromosphere may appear doubled, since local decreases of density from exterior parts to central parts of the sun may also occur. Indeed, every photograph taken by the Dutch expedition to Sumatra in 1901, on the occasion of the sun's eclipse, showed a doubling of all chromosphere lines. HARTMANN, of Potsdam, however, believes that the doubling of the lines is produced by errors of the apparatus.

Finally, when an observer looks at only the parts between aa and bb (or a_1a_1 and b_1b_1) of the whole phenomenon (Fig. 7) he observes only two broad bright bands of light, *the position of which is different from that of the D lines*. Earlier theory assumed this deviation to be produced by the Doppler principle, by supposing gas eruptions with a velocity of 500 kilometers per second. These enormous velocities astonished all astrophysicists, but they were finally believed, since no other explanation existed.

If these enormous velocities are due to a sudden rise of temperature, calculations based on gas theory show that a temperature of five million degrees would be necessary to impart to the molecules of hydrogen a velocity of only 200 kilometers per second. Anomalous dispersion gives an explanation of the observed displacements without any restrictions.

The phenomenon most difficult of explanation of all observed since the spectroscope was invented is the following: In the spectrum of one and the same prominence, or at one and the same place near a sun-spot, the bright lines of one element show distortion, while those of another element are fairly straight, and it is most astonishing that even *the different lines of one and the same element show a quite different appearance* (Fig. 9). Some are deviated and curved, others are in their proper place and are straight lines.

In order to explain this curious fact, Sir N. LOCKYER introduced the hypothesis that on our sun the elements are dissociated into elementary atoms. This idea sounded at that time somewhat ridiculous; but now, after the discovery of radium and the transformation of its emanation into helium, etc., we must admire Lockyer's imagination and

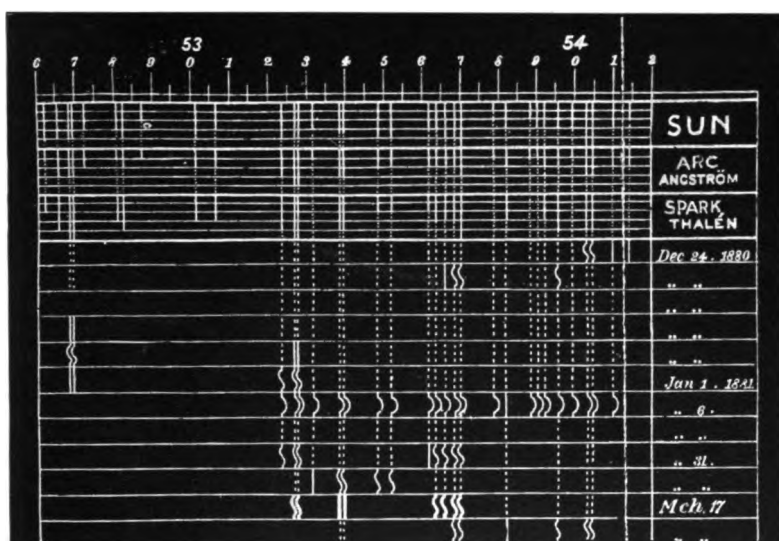


Fig. 9.

courage. On the basis of Julius' theory, I believe that this phenomenon is due to the different amount of anomalous dispersion of the different waves emitted by one and the same element.

Thus we see that, starting out from questions of purely scientific significance like black radiation or, what is the same thing, the determination of the constant S in Kirchhoff's law

$$\left[\frac{E}{A} = S \right]_{\lambda, T},$$

we have succeeded in answering questions of thoroughly practical importance. Much still remains to be done, and I venture to express the hope that by my address new problems may have been suggested to you, the solution of which will be found in your admirable institutions and laboratories at Washington.

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THE INSTRUMENTS AND METHODS OF RESEARCH

BY

L. A. BAUER

ADDRESS OF THE RETIRING PRESIDENT, DELIVERED BEFORE THE SOCIETY,
SATURDAY EVENING, DECEMBER 5, 1908

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THE INSTRUMENTS AND METHODS OF RESEARCH.

BY

L. A. BAUER.

(Address of the retiring President, delivered before the Society, Saturday evening, December 5, 1908.)

Were I to accuse you of forgetfulness, of shortness of memory, or possessed of that quality apt to prove troublesome to others, though characterized by the oldest of our past presidents, in his delightful "Reminiscences of an Astronomer," as a valuable quality—absent-mindedness—I dare say you would not be much offended, though possibly a trifle annoyed. But were I to accuse you of *narrow-mindedness* I might meet with a different reception. To none of us would it matter much to be called short-memoried or absent-minded, but to be termed *narrow-minded* arouses our resentment immediately. But are we not all necessarily so, more or less, according to the circumstances in which we find ourselves?

MIND THE CHIEF INSTRUMENT OF RESEARCH.

I believe it was the mathematical physicist Stokes who said we must not forget that the chief instrument of investigation—the mind—is itself the object of research. To the Mind, then, we should devote our first and chief attention in the discussion of the subject set for this evening. How to reduce and check as far as possible this natural tendency of all of us to narrow-mindedness in one or more directions, or how, realizing its necessary existence, to make due allowance for it in the formulation of conclusions which, though

drawn with utmost care, are nevertheless subject to a "personal equation," is, as we at once readily see, a matter of the very highest importance.

Many of you are doubtless familiar with the Hindoo fable set to rhyme by Saxe:

"It was six men of Indoostan
To learning much inclined,
Who went to see the Elephant
(Though all of them were blind),
That each by observation
Might satisfy his mind.

"The *First* approached the Elephant,
And happening to fall
Against his broad and sturdy side,
At once began to bawl:
'God bless me! but the Elephant
Is very like a wall!'

"The *Second*, feeling of the tusk,
Cried, 'Ho! what have we here
So very round and smooth and sharp?
To me 'tis mighty clear
This wonder of an Elephant
Is very like a spear!'"

The third, happening to grasp the "squirming trunk within his hands," declared the elephant to be "very like a snake;" the fourth, feeling "about the knee," thought the elephant seemed "very like a tree;" the fifth, "chancing to touch the elephant's ear," described him as being "very like a fan," and when within the scope of the sixth came the swinging tail, the fact that the elephant "is very like a rope" was to him proved beyond dispute.

"And so these men of Indoostan
Disputed loud and long,
Each in his own opinion
Exceeding stiff and strong,
Though each was partly in the right,
And all were in the wrong!"

And now, if you will permit me to slightly alter the poet's last verse, so as to point the moral to our own selves:

"How oft in scientific wars
We disputants are seen
To rail in utter ignorance
Of what each other mean,
And prate about an Elephant
Not one of us has seen!"

WHAT IS RESEARCH?

In this day of encyclopedias numerous and ponderous, one is often struck with the fact that in spite of the manifest care and conscientious thought bestowed by the responsible editors, the omissions and evidences of discontinuity of treatment, and lack of recognition of the prime purposes of the compilation are as noteworthy as the imposing array of the results of our steadily advancing knowledge is startling. For a philosophic treatment—one fully appreciative of that which the student really requires, not only to enlighten him with regard to a particular subject, but also to stimulate him to research where it is most needed—I frequently get more satisfaction out of the older encyclopedias than from our modern ones, even though they can but present the status of the subject up to the time they were written.

As an illustration, take the word "research," appearing in our topic of this evening, or any of the associated terms—"discovery," "experiment," "investigation," and "observation." Turning to the index volumes of the ninth and tenth editions of the "Encyclopædia Britannica," I find but two references in which the word "research" appears—one to the exploring vessel, the "Research," and the other to "research degrees." Turning to the page on which the latter occurs, we find this interesting statement referring to Oxford University:

"New degrees for the encouragement of research, the B.Lit. and B.Sc. (founded in 1895, and completed in 1900 by the institution of research doctorates), have attracted graduates from the universities of other countries. In 1899 a

geographical department was opened, which is jointly supported by the University and by the Royal Geographical Society." Now comes the interesting statement which I beg to emphasize: "*Of more bearing on practical life are the Day Training College Delegacy (1892) and the diploma in education (1896). Under the former elementary school teachers are enabled to take their training course at Oxford, and do so in growing numbers,*" etc.

We thus see what the writer of this article thinks of the relative value in practical life, of research foundations and normal school foundations! Yet we all know that this view is not typical of that held in a country having such productive research organizations as the Royal Society or the Royal Institution. Sir Norman Lockyer, in his luminous inaugural address before the British Association for the Advancement of Science, in 1903, on the "Influence of Brain-power on History," says: "A country's research is as important in the long run as its battleships." Why, then, does not the standard encyclopedia of that country make space for a representative article on "research"?

Under "investigation" there also appears absolutely nothing. However, we have the "Investigator" ship, Investigator Shoal, Investigator Group, etc., but not a word about the general methods employed by "scientific investigators." And so it is with the word "discovery"—there is no reference whatsoever to an article on the general principles leading up to discoveries. Likewise with the word "observation." Though there are many references to observations of various kinds, there is no one article for setting forth the general principles of "observations" or the part they play in the discovery of fundamental facts. The same experience is had with regard to the word "experiment."

Now let us turn to an encyclopedia I invariably read with pleasure and profit; it frequently has supplied me with references to earlier work not to be obtained elsewhere. We shall find it instructive to us tonight, though the articles to which I beg to invite your kind attention were written three-fourths of a century ago. I refer to the classic Gehler's Physi-

kalisches Wörterbuch—the revised edition by the noted investigators Brandes, Gmelin, Horner, Littrow, Muncke, and Pfaff, in 20 volumes and published in Leipzig, 1825-1845. A veritable fund of information is found under the headings “Beobachtung” (observation) and “Versuch” (experiment). The article on “Beobachtung,” by the physicist Muncke, embraces 28 octavo pages. He shows the distinction between “Beobachtungen” (observations) and “Versuche” (experiments) to be that the former pertain to the perceptions of phenomena presented to us by Nature in her unmodified course, whereas in the latter—in the experiments—we are seeking to produce certain results or phenomena, more or less looked for, in order either to verify a law already known or to disprove one suspected of being wrong or even to discover a new one. Both classes of experiences are necessary for a piece of investigation or research work.

Thus, we may behold either visually or in some other way certain striking solar phenomena; these belong to the class of observations which we ourselves are unable to modify in any manner whatsoever. Continued observation may, however, reveal a certain law which by experiment in the laboratory, conducted along more or less definite lines, we may seek to imitate in the hope of getting some clue to the *modus operandi* of the observed phenomena. In this article on “Observations” the author treats in detail the various elements entering into correct methods of investigation, condition of the observer and of his senses, his being unbiased, character and errors of the instruments, errors of results, methods of increasing accuracy, representations of observations by graphs and formulæ, method of least squares, etc. He points out the mistake sometimes made, that an established formula satisfying the observed phenomenon within certain limits represents an actual law of nature.

The article “Versuch” (experiment) consists of 44 pages and is contributed by the astronomer Littrow. He shows that the most rapid development takes place in those sciences which afford the greatest opportunity for experimentation, referring, *e. g.*, to the slow and painful progress of the astron-

omer as long as he had to confine himself to mere celestial observations and the comparatively rapid strides which occurred as soon as some of the observed phenomena could be either imitated by, or be compared with, those derived by laboratory experiment. The investigator, he says, must be absolutely free from preconceptions and be careful, cautious, and unbiased in his interpretation of what his senses may reveal to him. He illustrates how Man, called jokingly "das Ursachenthier" (the animal ever bent on ascertaining the cause of things), proceeds in ferreting out the why and wherefore of observed phenomena, and how his methods of circumspection develop with the advance of knowledge.

Though Man cannot determine the "Endursachen," or ultimate causes of things, the field open to him to discover the laws governing phenomena or *vice versa*, classifying and enumerating those which follow a certain revealed law, is, nevertheless, still very large and sufficient to tax his energies. Witness, for example, the host of observed phenomena obeying the law of inverse squares!

The remaining sections of Littrow's article deal with the reduction of the experiments to the laws of motion, the numerical expression of the observed results in definite units, the importance of the part played by instruments or mechanical appliances, derivation of laws governing the observations, methods of ascertaining these laws, methods of reduction and of publication, and errors to be avoided.

These two articles will show sufficiently the character and scope of similar ones we should like to see in our standard English and American encyclopedias.* Such information is contained in some measure, at least, though not as comprehensively, in the modern German book of reference, Brockhaus's "Conversations-Lexikon," as also in the "Grande Encyclopædia" of the French. It is truly remarkable that there should be such an oversight in our "International Encyclopædia," when it is remembered that the editor-in-

*Chamber's Encyclopædia is found to contain a short article on "Experiment;" also one on "Observation."

chief was one to whom research work owes a very great debt of gratitude indeed—the late and greatly lamented Daniel Coit Gilman. The only article found is one on “expert,” and this pertains chiefly to “expert evidence” in courts of law. Yet what better statement concerning the “research or scientific spirit” could be made than contained in the following quotation* from Gilman’s writings?

“It is perpetually active. It is the search for the truth—questioning, doubting, verifying, sifting, testing, proving, that which has been handed down; observing, weighing, measuring, comparing the phenomena of nature, open and recondite. In such researches, a degree of accuracy is nowadays reached which was impossible before the lens, the balance, and the metre, those marvelous instruments of precision, had attained their modern perfection. Wherever we look we may find indications of the scientific spirit. The search after origins and the grounds of belief, the love of natural history, the establishment of laboratories, the perfection of scientific apparatus, the formation of scientific associations, and the employment of scientific methods in history, politics, economics, philology, psychology, are examples of the trend of intellectual activity. The readiness of the general government and of many State legislatures to encourage surveys and bureaus, the establishment of museums of natural history, and the support of explorations illustrate this tendency. Even theology feels the influence. The ancient and sacred proverb has been rediscovered—the letter killeth and the spirit maketh alive. I will go only to the edge of this disputed territory and shelter my own opinions behind those of a learned and devout prelate of the English Church (Bishop Walcott), whose words are these: ‘No one can believe more firmly than I do that we are living in a time of revelation, and that the teachings of physical science are to be for us what Greek literature was in the twelfth century.’” * * *

“With the growth of the scientific spirit grows the love of truth, and with the love of truth in the abstract comes the love of accuracy in the concrete.”

Our foremost English dictionaries are in general not any more satisfying or edifying regarding the precise meaning

*Extracts from the “Launching of a University,” 1906, pp. 147-150.

of "research" in the scientific sense than are the standard encyclopedias. Their illustrations of the use of the word are usually neither apt nor sufficiently comprehensive.

HOW MAY WE SHARPEN OUR SENSES?

Of the senses, *sight* plays the greatest part in investigation. To this organ we have thus far devoted most attention to supplement and increase its natural powers by mechanical means—the telescope, microscope, etc. Next would rank the sense of *hearing*; but the appliances for increasing our sensations in this respect are comparatively few, and still more is this the case with regard to the senses of *taste*, *smell*, and *touch*.

Yet what truly wonderful powers of touch are developed by the blind, and how extraordinary are the capabilities of certain animals for foretelling the distant approach of a deadly foe by the means of hearing or of smell!

Might not Man also, to his advantage, increase or stimulate his less-used senses in some manner, to the same degree or approximately so, as that of sight? If he did, is it not possible that thereby he might have perceptions which would materially assist him in solving some of the vexed riddles of the universe? May he not for lack of proper development of these senses be in much the same plight as the "six blind men of Indoostan who went to *see* the elephant, though all of them were *blind*"?

If there is some possibility in this direction, how about the power of stimulating or interpreting our muscle sensations, the sensations of heat and cold, of pain, of pleasure, etc.?

Efforts have been made, as you know, to trace a definite connection between certain atmospheric phenomena and bodily sensations, or between the sensations to commit suicide or other crimes and certain meteorological conditions. Likewise are there attempts by well-known men of science to sharpen and interpret the psychic sensations.

There is revealed here a field of research but little explored

as yet—the increase of our powers of perception along other lines than chiefly those of sight. No one can foretell the future possibilities in these directions.

The doctrine of evolution teaches the result of long-continued use of any particular organ, and has familiarized us with the wonderful achievements of Nature brought about by sustained and continued effort along some definite direction. Both the physical and the psychic condition of the observer requires their highest and healthiest development to insure not only the best results with the ever-increasing accuracy or precision required by the steady advance of knowledge, but also to bring about that round- or broad-mindedness needed for the proper interpretation of the results observed.

THE MATHEMATICAL INSTRUMENTS OF RESEARCH.

A good-sized chapter might be written on the "Mathematical Instruments or Tools of Research." The predominating tendency of resolving or expressing every natural phenomenon—periodic or otherwise—by a Bessel or a Fourier series or by spherical harmonic functions has brought about at times, especially in geophysical and cosmical phenomena, if not direct misapplications, at least misinterpretations of the meaning and value of the coefficients derived.

Like a certain class of "naturalists," we also may have laid ourselves open to the opprobrious term of "Nature-Fakir," and instead of clarifying the situation our calculations may have actually contributed instead to "befog" it.

Frequently by the purely mathematical process there have been eliminated, in the attempt to represent a more or less irregularly occurring natural phenomenon by a smoothly flowing function, the very things of chief and permanent interest. The normal or average diurnal temperature curve, for example, or a uniform magnetic distribution over land, so as to yield perfectly regular lines of equal magnetic declination, never occur in Nature. There is thus being impressed upon us more and more forcibly the fact that what

we have been regarding as "abnormal features"—the outstanding residuals between observations and the results derived from the mathematical formula—are in truth not "abnormal" from the standpoint of Nature, but are rather to be taken as indicative of the "abnormality" or "narrow-mindedness," which means the same thing, of ourselves in trying to dictate to Nature the artificial and regular channels she should pursue in her operations.

Louis Agassiz said:

"The temptation to impose one's own ideas upon Nature, to explain her mysteries by brilliant theories rather than by patient study of the facts as we find them, still leads us away."

The fundamental law of Nature is to invariably follow the paths of least resistance, and by examining these lines of structural weakness of the opposing systems we may have opened to us the very facts which are to be of real value and of sure benefit to mankind. The irregularity of the banks bordering a natural watercourse serves to differentiate the work of Nature from that of the builder of the artificial and regular channel.

No, instead of rejecting, we must learn to retain the outstanding residuals and study them most carefully and regard *them* as the true facts of Nature, and not those which we so egotistically and presumptuously try to force on her. What great discoveries may lie open to us when we once have grasped the true significance of the facts we have been so fond of measuring by our own standard and have been terming as "abnormal" or "irregular"!

An interesting example of not wholly successful application of the continuous and ever-recurring functions of spherical harmonics to a typical geophysical phenomenon—the distribution of magnetism over the earth's surface—has been discussed by the speaker elsewhere. Though the number of unknowns has been increased in recent computations from the original 24 of Gauss to 48, nevertheless the difference between theory and observation is of such an order of

magnitude as to preclude the use of the formula for even the purely practical demands of the navigator and surveyor. Nor has any one succeeded in giving any physical interpretation of the laboriously derived coefficients beyond the first three. And what do these three stand for? The simplest possible case of a first approximation to the actual state of the earth's magnetism, viz., that of a uniform magnetization about a diameter inclined to the axis of rotation!

The prime difficulty here may be summed up in a word. The very surface over which the spherical harmonic functions are spread is itself such a prolific source of disturbance as to cause effects embracing a continent, a state, or a locality. Such a large number of terms would be requisite for an adequate representation as to make their computation prohibitive. We are dealing here with more or less discontinuous effects that cannot be imitated by continuous functions without leaving behind a train of residuals, precisely as though we were to try to fit to the actual configuration of the earth some standard pattern of our own. Let me ask what phenomenon have we, in fact, which will admit of the determination of 48, or even of 24, physical constants?

It had been my intention to say a few words on the value and limitation of that much-used as well as abused mathematical instrument of research, the method of least squares. Properly employed, it is a most useful adjunct to investigation; but, as intimated, the true significance of formulæ established by this method is at times pushed way beyond the limitations. What the tenor of my remarks might be will be sufficiently evident to you if I submit this query for your consideration: *What actual laws of Nature have been discovered by the method of least squares?*

THE MECHANICAL INSTRUMENTS OF RESEARCH.

A few minutes were to have been given to the instruments employed by the scientific man to sharpen and amplify his natural senses and sensations—in a word, the tools furnished him by the mechanician. I am glad, however, both for

your sake and mine, that this part of the subject was covered by an interesting paper presented at the previous meeting of the Society. It was emphasized there that for the best results it is essential that the investigator be able to work with instruments so constructed as to permit him to control or renew the various adjustments without the necessity of returning the instrument to the maker. The principle at times employed, which assumes that when adjustments are once made they are to "remain put," is a very pernicious one. A number of very interesting examples from my own experience in the purchase and manufacture of magnetic instruments during the past ten years might be cited; but, as has been said, this part of the subject having already been covered, there is no need to dwell further upon it than to emphasize the injunction that the research worker, if he desires the best results, must know his instruments as thoroughly as himself.

SUBJECTS OF RESEARCH.

We come next to a brief consideration of the *subjects* of research, though not specifically mentioned in the title of our paper, yet implied in it. The rapid progress made by a science as soon as it reaches the stage of experimentation has already been noted. A crucial experiment has at times furnished information which by mere observation of phenomena, running their natural and unmodified course, might either have never come into our possession or at best would have taken a considerably longer time than that of the decisive experiment. You are all familiar with such cases, for almost every science can furnish examples.

Now it is an extremely interesting and suggestive fact that the greatest experimental discoveries today are not made in the older, well-recognized sciences, but on their borderlands—in the "twilight zone" of more or less related sciences. I have but to mention the words "physical chemistry," "physical geology," "astrophysics," "biochemistry," etc., and you will readily grant the assertion made. In the overlapping regions there seem to be the greatest opportunities afforded

for solid, thorough, and at the same time remarkably rapid, experimental achievements. And so we are having produced almost daily new specialties or new sub-specialties.

What is the effect on the general broad-mindedness of Man by this extreme specialization, so necessary for the production of the best and most far-reaching results? *Is the modern specialist more narrow-minded than the generalist of a century or two ago?* In view of our opening statements that the prime instrument of research is, after all, the mind, the question is, as you see, not an irrelevant one. We find statements occasionally made which would imply an affirmative answer to our question; but I, for one, would most emphatically protest against such an inference. I should maintain that the specialist, other things being equal, is likely to be a broader man than he who has no specialty, but simply a general knowledge of some particular science. The reason for my positive statement would be found in the fact mentioned, that the greatest part of the research work today is being done on the border-lands of the general sciences; for he who wishes to take part in this very active competition must needs be far better equipped than the mere generalist. The physical chemist, to be most successful, must have a very intimate knowledge of both physics and chemistry, and the more mathematical skill he possesses the better. The astrophysicist must be a physicist, a chemist, a mathematician, besides being an astronomer. And so with regard to the geophysicist.

Only a few names need be cited—like those, for example, of Faraday, Maxwell, Kelvin, von Helmholtz, Mascart—to support the contention that the broadest physicists are, as a rule, those who have regarded their laboratory experiments and deductions therefrom merely as a means to an end, not an end in themselves, and who have accordingly sought to apply the knowledge gained to the solution of some of the great problems affecting the general welfare of Man. There is the greatest need in this country of well-trained and well-equipped physicists in the solution of the many perplexing problems of the earth's physics with regard to the phenomena

of seismology, vulcanology, meteorology, atmospheric electricity, terrestrial magnetism, etc., etc. When the investigator makes the attempt to apply some of his laboratory facts to geophysical and cosmical phenomena, he has opened to himself a world of which he never dreamed; he finds zest in familiarizing himself with the fundamental facts of other sciences in which until now he could take no interest.

METHODS OF RESEARCH; DISCOVERY OF LAWS.

The methods in general have already received treatment in connection with the foregoing topics. It is always interesting to know what was the precise course followed in the discovery of a great law. However, no two investigators have ever pursued, or at least but rarely, precisely the same paths, and we must therefore be content with the statement of the general principles of research, such as has already been given.

A prevalent fault is observed in scientific publications whenever the investigator has had good training only on the observational side and but very little experience in scientific computing. He is very apt to violate one of the first and fundamental principles of good observing, viz., to employ such a method or scheme of observing as will yield but one definite result, and that with the highest possible accuracy and with the least amount of computation. Oftener than may be thought, schemes of observation are used which leave an arbitrary element to the computer, and in consequence a different result is forthcoming, according to who makes the computation. Had we time apt illustrations could readily be given from published works. The point made, that the observer must also bear in mind the computation side, and work up his results as soon as possible, is of fundamental importance in research work.

It may be worth while to consider briefly the insatiable desire of the analyst to ring in a series of sines and cosines to resemble the course of some natural phenomenon of which he does not know the exact law. Is this the old story over again, though in somewhat altered garb, of the epicycles and deferents of ancient astronomical mechanics, which re-

ceived its highest development in the Ptolemaic System of the Universe? You will recall that Ptolemy, building on the suggestions of Apollonius and of Hipparchus, supposed a planet to describe an epicycle by a uniform revolution in a circle whose center was carried uniformly in an eccentric round the earth. By suitable assumptions as to his variable factors, he was thus able to represent with considerable accuracy the apparent motions of the planets and to reproduce quite satisfactorily other astronomical facts. This was the artifice employed by the astronomer of the period before the modern and more subtle art of simulating Nature, by the sine-cosine method, had become known.

What seemed so intricate and complex in Ptolemy's time could be expressed in very simple language indeed, when a Kepler discovered the true functions as embodied in his three fundamental laws. The present method of hiding our ignorance of the real law, which seems at times to exert such a mesmerizing influence over us as to make us mistake the fictitious for the real, reminds one of the old conundrum: "Patch on, patch on, hole in the middle; if you guess this riddle, I'll give you a golden fiddle."

If the sine and cosine of the angle does not represent the curve of observation, patch on a sine and cosine of twice the angle; then, if necessary, thrice the angle; next, four times, and so *ad infinitum*! Now guess the riddle!

Of course I do not mean to discard this useful and in fact indispensable tool of research, but simply wish to call attention to its limitations and to the importance of not overlooking the fertile byproducts, the residuals, which, because of our neglect of them, may some day rise and smite us in their wrath. Each one of us at one time or another has doubtless established, by least squares, an empirical formula of some kind which so beautifully fits the observations as to make us bold and venturesome. Now comes a new observation, somewhat outside of the range for which the expression was established. Eagerly the test is applied, and we find to our chagrin that the formula on which so much work had been spent will not fit the new result, and that we have a "counterfeit" and not the real law.

Let us suppose, for illustration, we are dealing with a phenomenon which almost entirely unfolds itself during the time between sunrise and sunset—the well-known diurnal variation of the earth's magnetism is a striking case of the kind. Following the usual method, the phenomenon is resolved into component parts with the aid of a Fourier series. The formula as generally adopted includes the four terms having, respectively, periodicities of 24, 12, 8, and 6 hours. For ordinary magnetic latitudes the striking result is obtained that the second term—the 12-hour one—is as important as the first, or 24-hour, one; so we might equally as well say “the semidiurnal” as “the diurnal variation of the earth's magnetism.” In fact, as the semidiurnal term unfolds itself twice in 24 hours, it is in reality more important than the purely diurnal one.

Does the resolution into Fourier terms of a phenomenon of the kind given really prove their existence in Nature? Can we conclude, without question, *e. g.*, that in addition to the diurnal term we also have a semidiurnal one? Even with four terms, the series does not represent each hourly observation of the 24 with the same degree of precision. In fact, the residuals for the night hours are nearly of the same order of magnitude as the observed quantities. If the physical existence of the 12-hour term is not proved, then there is no need of racking our brains as to its physical origin.

The difficulty disclosed by this example is of the same kind as the one treated in spherical harmonics, *viz.*, that we are attempting to represent a discontinuous function having a duration commensurate with that of the daylight hours by functions running smoothly through their individual courses for 24 hours.

Babbage, the inventor of the calculating machine named after him, said he once had the following question put to him: “Pray, Mr. Babbage, if you put into the machine wrong figures, will the right answer come out?” Do we not at times attempt to put wrong premises into Nature's machinery and then expect correct answers?

We cannot close this section better than by quoting the

following passage from the address of the first President of this Society, Joseph Henry, given on November 24, 1877:

"The general mental qualification necessary for scientific advancement is that which is usually denominated 'common sense,' though, added to this, imagination, induction, and trained logic, either of common language or of mathematics, are important adjuncts. Nor are the objects of scientific culture difficult of attainment. It has been truly said that the 'seeds of great discoveries are constantly floating around us, but they only take root in minds well prepared to receive them.'"

Henry's insistence on the application in our scientific work of "common sense" reminds one of Clifford's apt definition of science as being "organized common sense."

PUBLICATION OF RESULTS OF RESEARCH WORK.

We come next to the question of publication of the results of research. I think it may be taken as almost axiomatic that whatever is worthy of investigation should be made known in some effective manner, so as to reach without question those concerned. The multiplicity of literature on any one subject or even on any small portion thereof is nowadays such that the worker finds it utterly impossible to keep abreast of publications, even those in his own field, to say nothing of kindred ones.

He is forced more and more to rely on abstracts—at least in so far as to direct him to that which he unquestionably must consult in the original, if possible. In my own particular line of work I rarely find that an abstract supplies all that is needed, and I almost invariably prefer to work directly with the original. I have heard similar statements from workers in other fields.

If it be true, then, that the investigator usually finds it necessary to consult the original publications, the next conclusion to be drawn is that the publication of any research work should, in general, be of such form and size as to permit the widest distribution possible, not only among the libraries and the principal seats of learning, but also among the workers and institutions immediately interested.

The scientific worker generally does not possess the means to purchase or to construct the instruments he requires for the prosecution of his work, and a book bearing in any way on the line of work to be pursued is as much to be considered part of his equipment as the purely mechanical tools. Indeed, I was told by the late von Bezold that Wilhelm Weber set his laboratory students to work by telling them, "Here are the instruments, and there are the 'Annalen der Physik'; now go to work." The man of science usually wants his tools close by and within ready reach. He cannot afford to go to a distant library and then possibly find the book out. Private possession permits him, furthermore, to make marginal notes and references to enable him quickly to put his finger on the very thing needed.

Owing to these well-recognized needs, there has grown up a courteous and friendly interchange of publications among coworkers and sympathizers in the same field that to my mind deserves the highest encouragement. The time has unfortunately gone when scientific investigators can write such delightful and voluminous letters as passed between the research workers of half a century and more ago. The present system of interchange of publications has necessarily taken the place, to a very large extent, of the early letter-writing. It is a system of gradual development along the lines of least resistance that it would be disadvantageous to contend against until some more effective means of intercommunication among scientific men has been devised.

But such free interchange of research publications can only be conducted to a limited extent and can embrace only certain kinds of publications, viz., generally reprints or those of which the original cost for publication has already been borne in some manner, be it by a journal or by some research foundation. Larger publications, however, because of their expensiveness, must generally be restricted, for one reason or another, in their general circulation, with the inevitable result that the persons directly reached may be entirely out of proportion to the importance of the work undertaken.

Scientific men and scientific bodies could well afford to pause and consider the tremendous cost of publication and

the rather frequent waste of money incurred. Why is it, for example, that when an explorer gives an account of his travels in an unexplored region for the commercial press he finds it possible to say what he wishes in an attractive and handy octavo form, but when he is working for an endowed institution he feels compelled to present his matter in an expensive, ponderous, quarto form, inconvenient to handle?

It should be noted that it is as important to make research work known as to do it. To get our friends to read the contributions we may make to science requires nowadays no little skill and diplomacy and an attractiveness of literary style on the part of the author not so essential in the days of less frequent printed works. The original purposes of important and costly expeditions are sometimes well nigh defeated or superseded, because of the delay in publication, ensuing from the elaborateness of the plan adopted for the reduction of the field results and the form of publication decided upon.

Reduction in the pretentiousness, size, and cost of scientific publications appears to me to be one of the greatest needs of research today.

METHODS OF RESEARCH BY INSTITUTIONS.

Some time could profitably be spent on a consideration of the general agencies engaged in furthering research work and the methods employed for doing so. Being, however, connected with a "research institution," I should consider myself incompetent to enter upon a free and unbiased discussion of the methods of such organizations for the furthering of research work. My remarks will be chiefly confined to a brief discussion of the methods to be used in certain investigations of a world-wide character. Craving your indulgence once more, I shall take as an example the general magnetic survey of the earth as representative of the kind of world-embracing research enterprises I have in mind.

Alexander von Humboldt, whose mental grasp was extraordinary in more than one science, set forth the following

plan in his "Cosmos" for a general magnetic survey of the globe.*

*"Four times in every century an expedition of three ships should be sent out to examine as nearly as possible at the same time the state of the magnetism of the earth, so far as it can be investigated in those parts which are covered by the ocean. * * * Land expeditions should be combined with these voyages." * * **

"May the year 1850 be marked as the first normal epoch in which the materials for a magnetic chart shall be collected, and may permanent scientific institutions (academies) impose upon themselves the practice of reminding, every twenty-five or thirty years, governments, favorable to the advance of navigation, of the importance of an undertaking whose great cosmical importance depends on its long continued repetition."

Here was a noble project, universally conceded to be not only of the greatest scientific interest, but also of the greatest practical importance. Yet why is it that this grand plan has never been carried out by the foremost nations in friendly concert? Have our Academies, as Humboldt suggested, never "imposed upon themselves the practice of reminding every twenty-five or thirty years governments, favorable to the advance of navigation, of the importance of an undertaking" of this character?

Instead of working along a common and definite plan, the magnetic operations hitherto have consisted of more or less isolated and incomplete surveys, independently undertaken by various nations and distributed over a great number of years. Not even for a single epoch has it been possible to construct the magnetic charts on the basis of homogeneous material, distributed over the greater part of the earth, with some attempt, at least, at uniformity. And as to the possibility of constructing the charts, with the aid of similar data, for epochs 25-30 years apart, as Humboldt had dreamed, this, in spite of the enlightened interest of many countries, is even more remote.

*The quotation is from E. C. Otté's translation of the *Cosmos*, vol. II, pp. 719-720.

Why should it have remained for a purely *research* organization to undertake a problem touching so keenly, as this, on even the so-called sordid, purely practical interests of Man? Is it a fortunate fact that Humboldt's fascinating international scheme failed of execution, and that the chief brunt of the work is now being borne by a single organization? It is not for the speaker to attempt to answer. The magnetic work of the Carnegie Institution of Washington has embraced, since 1904, a general magnetic survey of the Pacific Ocean, and land observations have been made in more or less unexplored regions in different parts of the world. The ocean magnetic work is to be undertaken next in the Atlantic Ocean, in 1909, on a specially built vessel, the first of its kind.

It is believed that an effective scheme of operation has been evolved, with the aid of the valuable advice received from eminent investigators. Without danger of giving offense to any one, it is possible to deal directly with the officials concerned, submitting to them our plans and ascertaining whether they contemplate doing anything similar, and, if so, whether, in case their funds are insufficient, they could suggest some friendly basis of coöperation between their organization and ours. This plan of action has met with entire success thus far. Duplication, overlappings, and possible jealousies are all avoided; and in countries where no organization whatever exists to do the work, we are free to go ahead and finish the task in less time than it would necessarily take to get an official action or official consensus of opinion from a large scientific body.

Slow deliberation in terrestrial magnetic work would be disastrous, for the prime reason that the phenomena of investigation in this field of research are continuously undergoing change. The time-element in the earth's magnetism, even for a period of a few years, is of such moment as to completely mask the fine, hair-splitting points which would necessarily and rightly have to be raised on some international mode of action, to say nothing of the painful and cumbersome method which would have to be employed to

conform with the rules of official correspondence between nations. Many a well and carefully executed magnetic survey in the past has had its full importance for world-wide investigation destroyed because of the possibility of error in the secular variation corrections which must be applied to bring its results up to the date of the later data.

Though I may be judged guilty of defending my own policy, I believe the course pursued by the Carnegie Institution of Washington in conducting the general magnetic survey of the globe is the only way in which this particular project, and similar ones to it, could not only be expeditiously conducted, but so as to realize the chief objects of the work. Judging from individual expressions received from scientific men everywhere, they appear in agreement with us. This policy, briefly stated, is: To make, with the aid of the friendly and harmonious coöperation of all concerned, a rapidly executed magnetic survey of the greater part of the globe, so that a general survey, all-sufficient for the solution of some of the great and world-wide problems of the earth's magnetism, will be completed within a period of ten to fifteen years. At a smaller number of points, selected in consideration of the prime questions at issue, the observations are to be repeated at intervals of five years or less, in order to supplement the rather sparsely distributed magnetic observatory data. Thus the determination of the corrections for reduction of the general work to any specific date is continuously provided for.

Now, had I the time or were this the place, I should like to add a paragraph regarding the needful accuracy and the prime questions to be considered in the conduct of such a piece of work. Suffice it to say that the most evident result of all magnetic work in the past is that, for the purposes of a general survey, it is far better to make some sacrifice in accuracy if thereby it is made possible to secure observations at another point. In other words, the errors due to local disturbing conditions are far greater than the purely observational ones. Hence multiplicity of stations rather than extreme accuracy and laborious methods of observation and reduction is the prime requisite.

STIMULANTS TO RESEARCH WORK.

Dr. Gilman, in his charming reminiscences of the non-resident lecturers of the Johns Hopkins University, related the following of the great mathematician Sylvester:

"Sylvester enjoyed stimulants—I do not mean such vulgar and material articles as alcohol and coffee. I never saw any indications that he cared for their support. But he loved such stimulants to intellectual activity as music and light, lively society, in which he was not called upon to participate. Once at a symphony concert I sat just behind him, admiring the dome of his capacious cranium, unconcealed by hair, and I noticed how absorbed he was. The next day, Sunday, he came to me impetuously to say that he had worked out some mathematical proposition at the concert of the evening before, the music having quickened his mathematical mind. He really thought this was his greatest achievement yet, and he had hastened to write it out and mail it to the Academy of Sciences in Paris. Once he told me that, having a special paper to prepare, he went to a store and bought a pound of candles, which he placed about his room, on all sorts of extemporaneous candlesticks; 'for light,' he said, 'is a most powerful tonic.'"

These anecdotes will serve to recall similar ones of noted men, and many of you, doubtless, were this an experience meeting, could easily occupy the balance of the evening in delightful recollections of what each has found best to stimulate him to renewed intellectual activity; and I dare say that many of you would unite with me in declaring that membership in this Society has been one of the most helpful and stimulating influences.

We really have much to be proud of in the history and membership of the Philosophical Society of Washington. I should, indeed, consider myself remiss in the duties imposed upon me by the subject selected did I not refer at least to the eminent part this Society, through its members, has taken in bringing about the wonderful appreciation of scientific work and scientific methods we are today witnessing in our country. There have been several notable addresses by past

presidents that might advantageously have been reviewed in connection with our topic. But we lack the time.

I cannot refrain, however, from quoting once more from Henry's address, already referred to, which I hope you may be induced to read in its entirety:

"Man is a sympathetic being, and no incentive to mental exertion is more powerful than that which springs from a desire for the approbation of his fellow-men; besides this, frequent interchange of ideas and appreciative encouragement are almost essential to the successful prosecution of labors requiring profound thought and continued mental exertion." * * *

"It is an essential feature of a scientific society that every communication presented to it should be subject to free critical discussion. Such discussion not only enlivens the proceedings, but is generally instructive, frequently eliciting facts which, though insignificant when isolated, when brought together, mutually illustrate each other and lead ultimately to important conclusions."

CONCLUSION.

My address began with statements revealing the necessity of keeping our minds ever open and free for the careful weighing and the unbiased reception of the facts observed and discovered. Throughout I have attempted to lay chief stress upon the mental and human elements involved in the topic. I cannot do better in closing than to quote you a sentence from a letter* which the great mathematical physicist, James Clerk Maxwell, wrote to Herbert Spencer on a subject of controversy in the latter's "First Principles," viz.:

"It is very seldom that any man who tries to form a system can prevent his system from forming around him, and closing him in before he is forty. Hence the wisdom of putting in some ingredient to check crystallization and keep the system in a colloidal condition."

Let our watchword therefore be: *ever to keep our systems—our theories—in a colloidal condition!*

* "Life and Letters of Herbert Spencer, by David Duncan," vol. II, p. 163.

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PHILOSOPHICAL SOCIETY OF WASHINGTON

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WILLIAM EIMBECK

BY

EDWIN SMITH

READ BEFORE THE PHILOSOPHICAL SOCIETY OF WASHINGTON

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WILLIAM EIMBECK.

1841-1909.

 (Read before the Society May 22, 1909.)

Mr. WILLIAM EIMBECK, the subject of this sketch, and myself were close friends for many years. His ambitions were well known to me, and I am very well aware that his failure to attain the final success he had hoped for was due to an organic disease which slowly crept upon him during the later years of his life.

He was born January 29, 1841, in Brunswick, Germany, and, beginning his education in the public schools and gymnasiums of his native city, he came to the United States at an early age and completed his training as civil engineer under private instruction in St. Louis. His first professional experience was in connection with the building of the Eads bridge at St. Louis, and in the offices of the St. Louis City and County Engineers. Later, for two years he was Professor of Mechanical and Civil Engineering in Washington University.

In 1869 Prof. J. E. Hilgard, the Assistant in Charge of the Coast Survey and later its Superintendent, had organized a series of parties to observe the solar eclipse of August 7 of that year. Mr. Eimbeck was an enthusiastic volunteer observer and was assigned to the party of Julius Pitzman, county engineer of St. Louis, and stationed near Mitchell, Illinois. After the eclipse he took part in the determination of the latitude and longitude of St. Louis and the connection of the various eclipse stations in Missouri and Illinois with this base station. His enthusiasm and success in this work led to his selection as an observer in the expedition organized by Prof. Benjamin Peirce, then Superintendent of the

Coast Survey, to go to southern Europe to observe the solar eclipse of December 22, 1870. Mr. Eimbeck was assigned to the party of Prof. C. H. F. Peters, the distinguished astronomer, with whom he observed the eclipse on Monte Rosso, near Catania, in Sicily. His ability, acquirements, and enthusiasm displayed on these two expeditions led to his appointment on the Coast Survey July 1, 1871, and his connection with it continued for thirty-five years.

His first assignment was to one of the triangulation parties on the survey along the 39th parallel of latitude, which was operating in Missouri, extending the work westward from the base in the Great American Bottom opposite St. Louis; and later he was engaged in astronomical duties in connection with determination of latitudes, longitudes, and azimuths in Kansas, Texas, and Louisiana.

In 1872 he was assigned to the Pacific coast, and for five years he was engaged in astronomical and primary triangulation work along that coast from Oregon to the entrance of the Gulf of California, one of his undertakings being a determination of the geographical coördinates and magnetic elements at thirteen stations between San Diego and Cape San Lucas for the control of the survey of the coast of Lower California, then in process of execution by the Navy Department. In 1872 in the Superintendent's report is an evidence of the thorough spirit in which he entered upon securing a thorough command of all the details of the scientific operations upon which he was engaged, this being shown by his paper suggesting improvements in the Hipp chronograph, then used in connection with the telegraphic longitude operations.

In 1877 he returned to the eastern coast, where he was assigned to an extensive astronomical and magnetic campaign for determination of latitudes, longitudes, and the magnetic elements in Kentucky, Illinois, Tennessee, South Carolina, and Georgia, and later, after making the necessary preparations, in 1878 he was again assigned to the western coast and began at Pah Rah, in Nevada, the extension of the primary work eastward from the coast triangulation, which

was to follow approximately the 39th parallel of latitude to the capes of Delaware. This was the inception of what was to be the main life work of Mr. Eimbeck, and to which for 18 years he gave all that was best in both mind and body. Stretching from the Sierra Nevadas to Pikes Peak and the east line of the Rocky Mountains, and including in its list of occupied stations mountain peaks reaching an elevation of 14,400 feet, in regions where supplies had to be carried for hundreds of miles through deserts and wastes, destitute of roads and almost destitute even of water, the successful conduct of this work called for the endurance of the most rugged of pioneers, the undaunted courage of the explorer, while the operations represent the highest type of work demanded from the scientist and observer. In this triangulation one line, observed in both directions, is over 183 miles long, and is not exceeded in the work of any country.

There are two instances where the change between adjoining stations necessitated a journey of 300 miles, one of these being the transfer of parties from Mount Ellen to Mount Tavaputs, made under the fierce suns of August and September, across a desert section which tested almost to their limits the endurance of the men and animals, and it is remarkable to relate that in his most expansive moments Eimbeck never seemed to consider that any special merit could be claimed for successfully overcoming all these hardships and dangers. A reference to the annual report of the Superintendent will emphasize this feature of our friend's character, as therein will be found only a simple statement of the work completed each year, because of the modesty which would not permit him to give an adequate account of the toils he faced and conquered.

Near the close of this great triangulation Mr. Eimbeck designed the Duplex Base Apparatus, which was constructed at the Coast and Geodetic Survey Office and used by him in the measurement of the Salt Lake Base.

In addition to the field work upon which he was so actively engaged, he was always deeply interested in every branch of the work of the Survey, and specially those per-

taining to geodesy. In 1900 he undertook an elaborate series of experiments for the study of the seasonal range in the value of the coefficient of refraction, but a final report was never received from him, although he made several announcements of the satisfactory progress he was making in the perfection of a theory for this important term. His study of the question of the existence of sensible tides in the earth's surface had also occupied his attention for many years, and was the object of study with him up to his last days. It is known that he gave profound study to the problems of the tides, gravity, the causes of the variations of latitudes, etc., and it is also known from his own statements that he was preparing his theories for publication. The most frequent references to Mr. Eimbeck by his intimate associates are appreciations of his suggestive and illuminating discussions of many of the problems that attract the physicist and astronomer, and all the problems to which the geodesist gives attention.

In appearance Mr. Eimbeck was the elegant and distinguished gentleman. Tall, erect, of fine proportions and handsome features, he was often admired as he walked the streets of Washington carrying an overcoat on his arm, which he disdained to wear even in the coldest weather. In his early days he was of robust health, but as early as 1890 he began to complain that he could no longer endure either the physical or mental strain of former years. A few years later he thought he had Bright's disease, but would not consult a physician. This disease, with the complications that so frequently come with it, slowly crept upon him till in 1906 he resigned from the Survey, hoping a life free from care would improve if not restore his health. It was too late; he gradually failed, and finally, on March 27, 1909, his death resulted from a stroke of paralysis.

Mr. Eimbeck was a founder member of the Cosmos Club, for thirty years a Fellow of the American Association for the Advancement of Science, a member of the Washington Academy of Science, of the National Geographic Society, of the

Geological Society, and of the Washington Philosophical Society.

In closing this sketch of the life of Mr. Eimbeck, I wish to quote from a little notice that was issued to the members of the Coast and Geodetic Survey a few weeks since:

“A lifetime of study and research, added to charming natural eloquence and marked clarity in exposition, made Mr. Eimbeck one of the most interesting and instructive of companions, these distinguished qualifications being set off by a modesty as extraordinary as the merits it failed to obscure. Broad and tolerant in his sympathies, and with no thought of self in his generosity to the unfortunate, the life just closed is one that can justly claim only praise when it is referred to, and affection when it is recalled.”

EDWIN SMITH.

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PHILOSOPHICAL SOCIETY OF WASHINGTON

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SIMON NEWCOMB

MEMORIAL ADDRESSES

BY

CHARLES K. WEAD

R. S. WOODWARD

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MILTON UPDEGRAFF

HERBERT PUTNAM

E. M. GALLAUDET

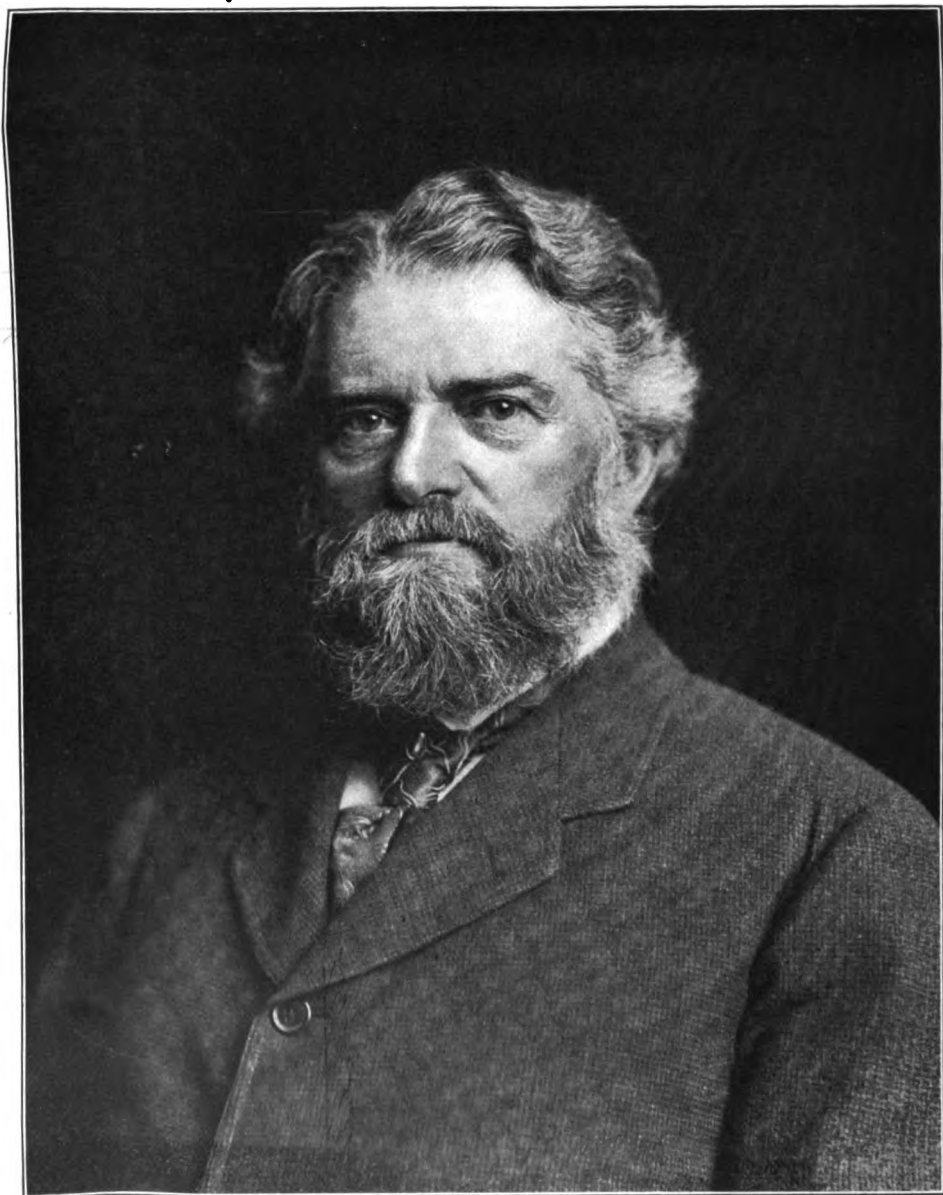
RESOLUTIONS READ BY O. H. TITTMANN

READ BEFORE THE PHILOSOPHICAL SOCIETY OF WASHINGTON,
DECEMBER 4, 1909

WASHINGTON
PUBLISHED BY THE SOCIETY
FEBRUARY, 1910

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AT A MEETING OF THE PHILOSOPHICAL SOCIETY OF WASHINGTON,
HELD DECEMBER 4, 1909.

INTRODUCTORY REMARKS OF MR. CHARLES K. WEAD, PRESIDENT OF THE SOCIETY.

We are met to commemorate the life and services of our late President, Simon Newcomb.

One of the by-laws of the Philosophical Society prescribes that the meeting preceding the annual meeting in December "shall be set apart for the President's Annual Address." It is, therefore, peculiarly fitting that President Newcomb's friends meet tonight as they would do if he were alive and well; and while we may not hear his voice, through the lips of others we shall be taught some of his knowledge and wisdom, and catch some inspiration from the story of his life-long devotion to lofty and difficult tasks. So we may feel tonight that he, like the martyr of old, "being dead, yet speaketh."

Professor Newcomb was one of the founders of this Society in 1871, and Joseph Henry was its only President till his death in 1878. At the next election Simon Newcomb was chosen to fill the vacancy, and held the office for two years: since then a single term has been the invariable custom. During his terms the Society became so large that the An-

thropological and Biological Societies were formed out of it. During 1907-8 Mr. Newcomb was prominently before the Society and presented several interesting communications, and a year ago was, for the third time, elected President.

The addresses this evening cannot, of course, be expected to present a chronological, complete or well-balanced biography; for the activities of his long and busy life have been too great for a single evening's presentation. Bibliographically these activities are represented by about a dozen independent volumes, more than half a hundred scientific memoirs and papers referred to in the Royal Society's Catalogue, and hundreds of magazine and newspaper reviews and articles, signed and unsigned, all duly listed by an admiring cataloguer. Tonight we may expect to hear of only a few of these activities, but of these few, somewhat in detail, and as seen from diverse view-points.

Running through all the important expressions of his life, I think we may see, more than in most men's, evidences of a profound belief in law; it appears in his orderly life, free from the proverbial eccentricities of genius, as well as in his monumental works based on the laws of gravitation; it underlies his work on the principles of political economy, his attempts to find the facts and laws of psychical phenomena, and to apply the laws of probability to stories of apparitions and coincidences.

To keep the facts presented this evening in right relations of time, it will be convenient to bear in mind a very few dates:

Born at Wallace, Nova Scotia, March 12, 1835.

Graduated, B. S., from Lawrence Scientific School in 1858.

Became a member of the American Association in 1859.

Appointed Professor of Mathematics in the United States Navy, 1861.

Married, 1863.

Elected Associate member of the Royal Astronomical Society, 1872, and since then elected to about forty societies in twelve foreign countries.

Invited to become Director of Harvard Observatory, 1875.

Superintendent of the Nautical Almanac, 1877 to 1897.

Lectured on Political Economy at Harvard, 1879 and 1880.

Professor of Mathematics at Johns Hopkins University, and editor of the American Journal of Mathematics for many years from 1884.

President of the Society for Psychical Research from 1885.

Retired with the rank of Rear Admiral, 1897.

As many of this audience were out of town during the summer, it is proper to add that Professor Newcomb died at Washington, D. C., July 11, 1909, after many months of severe suffering. Official honors were paid at his funeral, which was attended by the President and high officials of the Government; representatives of two of the foreign countries that had honored him in life were among the honorary pallbearers; he was buried with the ceremony prescribed for a Rear Admiral, the body being escorted to Arlington by several companies of marines and their band.

Tonight other foreign representatives honor the Society and its late President by their presence. One of them, a fellow-member in several learned societies and the Ambassador from the land of his birth, will now speak of his friend.

ADDRESS OF RT. HON. JAMES BRYCE, O. M.,

AMBASSADOR FROM GREAT BRITAIN.

Although there are many here present who knew Professor Newcomb more intimately than I did, and who are far more competent to speak of his scientific genius and the work he did for science, there is no one who admired that genius more warmly, or who comes more willingly to pay a tribute both to his splendid gifts and to the elevation of his character. Nor is it unfitting that such a tribute should come from one who is privileged to represent in this country both Nova Scotia, which gave birth to the illustrious man we commemorate and where he was known and honored, and Great Britain, to many of whose scientific societies he belonged as an honorary member.

All I can attempt is to place before you in a few words the impression which he made upon me in the intercourse of private life. That impression was primarily the impression of power. He had a noble head, piercing eyes, a look full of strength—steady, direct, penetrating. This gaze, with its quiet poise and a sort of reflective depth about it, reminded me of our other famous men of science of the last generation, Helmholtz and Adams, both mathematicians, and the latter also an astronomer. Nor is it fanciful to find that something which these three had in common is in the portrait also of one even greater—Sir Isaac Newton.

Professor Newcomb's conversation bore out what his aspect conveyed. Whatever subject he touched, what he said was weighty, for he had thought deeply on many things, some of them far removed from his own peculiar studies. He was always calm, lucid, and judicious. He saw things broadly, in their principles, and had that remarkable power of getting right at the heart of a subject, which is one of the most sure indices of a superior mind. Whatever subject he talked on, it became interesting in his hands, and I found him as luminous and judicious in discussing political or economic questions as he was in his own field of science. The honors showered upon him had not affected his simplicity or made him self-conscious, and his conversation was not only easy and pleasant, but sometimes touched with humor.

He struck me as one whose natural powers were so great and industry so unflagging that he must have attained eminence and distinction in any career to which he had devoted his talents. He might have been (so it seemed to me) a great engineer or a great financier; might have constructed vast works and piled up, had he cared for money, a huge fortune. But his mind was bent on higher things.

The intensity with which he loved knowledge made him adhere through a long life to the path which he had chosen—that of a student who was devoted to science for its sake, and who counted the discovery of new truths to be the finest kind of work and the highest kind of enjoyment. In this respect, as well as in the simplicity and uprightness of his life and in the courage with which he bore up against a

fatal malady, laboring to the last, he has left a noble example to us all and a name which will always shine among the brightest in the annals of American science.

ADDRESS OF MR. MILTON UPDEGRAFF,
DIRECTOR OF THE NAUTICAL ALMANAC.

Among the many honors conferred on Professor Newcomb, in recognition of his services to astronomy, is the Copley Medal of the Royal Society of London, which he received nearly twenty years ago. The award has recently been announced of the same medal to Dr. Geo. W. Hill, of West Nyack, N. Y., who was for many years an associate, assistant, and friend of Professor Newcomb in the Nautical Almanac Office. In commenting on this high honor again bestowed upon an American, the editor of one of our most ably conducted reviews states that the incident is "a matter for national gratification," and that "while it may be invidious to make distinction among different departments of research, yet a certain primacy belongs to that domain (Celestial Mechanics), which is at once the most ancient field of scientific inquiry and demands for its exploration all the resources of the most abstract and highly developed of sciences."

It was to the promotion in the broadest possible way of work in this field that Professor Newcomb for more than half a century gave his magnificent abilities, the weight of his character and his personal influence. When we consider the need that existed and the completeness with which it was met by Professor Newcomb, the greatness of his achievements and the magic of his name, greater in Europe even than in our own country, is explained.

His career in the intellectual world commenced when in 1857, at the age of 22, he became a computer in the Nautical Almanac Office, then located in Cambridge, Mass. In 1861 he was made a professor of mathematics in the Navy, and was transferred to the Naval Observatory at Washington. For several years he was engaged in practical astronomical work.

But he continued to devote his spare time to celestial mechanics, and later was for some years on special duty making investigations of the motion of the moon. In 1877, after twenty years of broad and active experience in theoretical and practical astronomy, he was made Superintendent of the Nautical Almanac Office, which in the meantime had removed from Cambridge to Washington. Thus were brought together the work to be done, the facilities and resources for doing it, and the man of all men to direct it.

At that time practical or observational astronomy had outrun the accuracy of the theories for predicting the motions of the heavenly bodies. The tables of the sun, moon and planets, and the fundamental constants of astronomy were in a chaotic condition. While sufficiently accurate for the needs of navigators and the ordinary land surveyor, the accuracy of the theoretical astronomy of the day was totally inadequate for the requirements of exact science.

As Superintendent of the Nautical Almanac Office, Professor Newcomb proceeded without delay to deal with this situation. He immediately made such improvements in the yearly volumes of the American Ephemeris and Nautical Almanac as were then practicable, secured modest but necessary increases in the appropriations for the work of the Office, and pushed with great vigor the work of "improving the tables of planets, moon and stars," as was and still is required by the law. He established a series of publications entitled "Astronomical Papers of the American Ephemeris," of which the first number, being a series of tables of eclipses, was published in 1879. Eight quarto volumes of these papers were published under the supervision of Professor Newcomb, and they contain tables of the sun and of all the planets and numerous researches in celestial mechanics and the fundamental constants of astronomy. In his preface to the first volume of the Astronomical Papers, published in 1882, Professor Newcomb says, page X, "On assuming the superintendence of the American Ephemeris in 1877, the writer determined to employ the resources at his disposal to carry out, or at least to enter upon, a long cherished plan of executing the work in question," more or less as outlined above.

In May, 1896, when this work was practically finished, a conference of the Superintendents of the National Ephemerides of the world was held in Paris, for the purpose of agreeing upon and adopting a uniform system of astronomical constants. Professor Newcomb was the leading spirit of this gathering. He was requested by the conference to prepare a catalogue of fundamental stars for use with the constants adopted. This catalogue was prepared and is published in Vol. VIII of the *Astronomical Papers*, and together with tables of the sun and planets and the constants adopted by the Paris Conference, is now used by all the National Ephemerides and by the astronomers of the whole world. Many decades must pass before the advance of astronomy will require that this work be superseded by new investigations.

It would seem at first sight that work of such great and fundamental importance should be more carefully fostered by the great governments of the world, and not left so much to the initiative of individuals. Astronomy, the oldest of the sciences, has been by far the most intimately connected with the intellectual as well as with the material progress of the human race. No civilization has, I think, ever developed without its system of astronomy. When Cortes conquered Mexico, the Aztec Calendar was superior to the Julian Calendar, then in use in Europe.

But governments are engrossed in attempting to minister to the more material and apparently more urgent needs of men. The progress of theoretical astronomy has always been dependent on the individual astronomer, sometimes with and oftener without governmental aid. In the main it has been the exceptional man who has produced unusual results in this field. But in the present state of scientific progress, in its growing complexity, both are needed in conjunction, and the resources of the government should be expended under the ablest possible direction.

This fortunate combination existed in the case of Professor Newcomb and the Nautical Almanac Office, and as regards practical achievements his place in history is secure as the

foremost astronomer of the 19th century. In the most progressive age in the history of the world, he occupies a place corresponding to that of the greatest astronomers from Hipparchus to Gauss, and appreciation of his work will increase with the passage of time. His constructive genius siezed upon every instrumentality for accomplishing results. That our country should produce such a man so early in its history is just cause for national self congratulation, and none the less that this distinction is justly appreciated and freely accorded us by the world.

A word should be said about Professor Newcomb's work on the motion of the moon, which he began when a young man, and continued at intervals throughout his whole life. It is well known that the lunar theory is the most difficult subject in the whole range of celestial mechanics. Professor Newcomb's work in this direction was largely of an empirical character. In 1861 he published a paper on the differences between the observed and predicted places of the moon, and early in the seventies he visited Europe for the purpose of securing and making use of old observations of occultations of stars for the purpose of extending backward our knowledge of the moon's motion. As a result of this work, Professor Newcomb was able to obtain corrections to the moon's longitude which have been used for many years to increase the accuracy of the predicted places. Though further research was necessary, pressure of other work made it impossible for this task to be completed in the Nautical Almanac Office prior to Professor Newcomb's retirement for age in 1897. Several years later he was able to take it up again under the auspices of the Carnegie Institution, and about a year ago arrangements were made to have the work finished in coöperation with the Nautical Almanac Office, and for printing the results as one of the Astronomical Papers of the American Ephemeris.

This work was nearly completed when, last May, Professor Newcomb learned that he had only a few weeks to live. Then in his enfeebled condition, and when suffering great pain, he applied himself to the task of directing the finishing touches;

and last June the manuscript was sent to the Nautical Almanac Office. The printer's copy is now being prepared, and the printing of this valuable and last work of Professor Newcomb would have been commenced some months since, but for a shortage in the printing fund of the Navy Department. The corrections to the moon's place are, however, available for use in the Office, and the elements of eclipses for the year 1912, as they will appear in the volume of the American Ephemeris for that year, have been improved by applying these corrections to the positions of the moon. As a result we expect that the predicted times of beginning and ending of these eclipses will prove to more nearly agree with the results of observation by about 15 seconds. Thus again, as he had so often done during a long and busy life, he added, as it has been fitly said, to "the only permanent wealth of nations."

However, it should be remembered that Professor Newcomb's astronomical work was not confined to remote and abstract regions incomprehensible to ordinary minds. As a writer on astronomy and allied scientific subjects, he has rendered the greatest service, not only to all grades of students, but to all intelligent readers of popular scientific literature. His profound knowledge, logical and orderly mind, together with a facile pen, made him the best popular writer on astronomy since Sir John Herschel.

For about a year and a half before his death, while the Nautical Almanac Office was engaged in certain work in coöperation with Professor Newcomb, I had frequent consultations with him. For several years previous I had known Professor Newcomb and have talked with him from time to time, usually on astronomical subjects. The breadth of his knowledge and the depth of his insight, and also the moderation which accompanied the firmness and precision of his judgments, were conspicuous. He was always open to conviction and would respectfully and patiently listen to opinions which he had perhaps little reason to value. He was kind and charitable, seemed always ready to recognize merit, and there is no doubt that many astronomers owe much to

him for acts of kindness unostentatiously performed, and, perhaps, in many cases entirely unknown to the beneficiary.

The severe and rigorous conditions which must be met by a man who would improve the work which has been done in celestial mechanics by the greatest minds in the history of exact science are conducive to an apparent, if not real, indifference to ordinary human affairs, but it is gratifying to think that a life so productive of such important results in a field not pervaded by humanizing influences, should have remained in the best sense so thoroughly a part of the age in which he lived.

ADDRESS OF MR. R. S. WOODWARD,

PRESIDENT OF THE CARNEGIE INSTITUTION OF WASHINGTON.

The task which your committee has assigned to me is a difficult one. Our colleague, Professor Newcomb, was engaged in scientific work for a full half century. The greater part of his labors was devoted to the applications of dynamical astronomy, or that branch of astronomy which deals with the masses and the motions of the planets and their satellites in our solar system. Since the publication of Newton's *Principia*, two hundred and twenty-three years ago, this field of scientific inquiry has been cultivated by nearly all of the scientific men of the world whose mathematical equipments were equal to the arduous requirements of this work. The field of activity we have to consider is therefore very extensive and very technical.

One of the peculiar obstacles to be met in attempting an adequate description of such a work arises from the fact that very few people, even in a select audience like the one here present, have given sufficient attention to the principles involved to enable them to form a just appreciation of the merits and of the achievements of the eminent men who have worked in this field. This obstacle does not arise, as is commonly supposed, from a lack of the requisite mental faculties for an appreciation of the physical principles in-

volved, or of the mathematical intricacies entailed by attempts to interpret and to apply these principles. It should be said, in fact, that neither any peculiar conformity nor any peculiar deformity of mind is required to appreciate the far-reaching principles of physical science or the recondite researches of pure mathematics. What is required, however, in a peculiar and unusual degree, is capacity for concentrated mental effort and sustained investigation, often to the extent of a whole lifetime, in order to comprehend adequately the fundamental postulates of matter and motion and the exquisitely refined machinery of analysis used by the leaders in mathematico-physical research.

A difficulty encountered in presenting anything like a just account of the achievements of our late colleague arises also from the wide range and from the vast quantity of work he accomplished. Much of this is so highly special, and requires for its interpretation so much time and attention, that only a few experts working along similar lines can be expected to be intimately acquainted with it. Hence what I may have to say to you in explanation of this work must be said mostly in general terms and from the points of view of one who has worked in closely adjacent fields rather than in the particular fields in which Professor Newcomb made so many weighty contributions.

On the other hand, a certain peculiar interest attaches to the subject you have assigned to me tonight, since our colleague was not only one of the most eminent astronomers of all time, but he was also one of those who have helped to make the astronomers of America the leaders in this branch of inquiry during the past half century. In general, we are obliged to admit that our country has not in the past produced its proper quota of eminent men in science, and amongst some of our fellow-countrymen it has been quite the fashion, ever since the publication a half century ago of the prediction of Alexis de Tocqueville that the conditions of life in America would be inimical to the development of eminence in science, to disparage both our capacity and our performance in this regard.

But whatever may be said with respect to science in general, I believe it to be a fact that Americans have maintained the lead during the past half century, both in theoretical astronomy and in observational and instrumental astronomy. In theoretical work the pre-eminence of Newcomb and Hill is universally admitted. In the field of instrumental astronomy it is well known that nearly all of the best and largest refracting and reflecting telescopes, along with their indispensable auxiliary apparatus, have been developed and constructed by Americans, while in the field of observational astronomy there has been equal leadership in the perfection of instrumental appliances and in the quality and quantity of work produced.

Before passing on to some account of his extraordinary achievements it is perhaps well to warn those who are unfamiliar with the domain in which he worked that Professor Newcomb would have been amongst the last to file any special claims to what the world would call "genius," that intangible, if not ghostly, something which in the popular mind lies close to witchcraft, and is often attributed to wizards, otherwise etymologically known as "male witches." It would be more correct to attribute to Professor Newcomb unusual talents for the kinds of investigation he undertook, unusual industry in applying these talents, and an uncommonly calm and sane mind for the estimation of premises and for the derivation of logical conclusions therefrom.

It should be stated likewise that he lived during a time peculiarly favorable for the researches he undertook. He also had the good fortune to receive early and continuously a just appreciation of his contributions to science from his eminent contemporaries, especially in Great Britain and on the continent of Europe. The association with these contemporaries arising from this early recognition furnished a healthy stimulus to his endeavors, while the continuity of effort supplied by a vigorous constitution enabled him to attain a degree of success in the application and in the prolongation of his labors rarely attained by men of similar abilities. All of these favoring conditions help to explain

the extraordinary quantity and quality of his contributions to astronomical science.

Early in life he set for himself the gigantic task of bringing the observed motions of the members of the solar system into harmony with the Newtonian law of attraction, and for nearly fifty years he was one of the small number of leaders in this large division of mathematical astronomy. The harmony in question had been demonstrated to the first order of approximation in that mightiest of all systematic treatises, the *Mécanique Céleste* of Laplace. But the vast accumulations of precise observations of the planets and their satellites during the first half of the nineteenth century showed the necessity for a higher order of approximation, which could be attained only by an insight and an industry comparable with those of Laplace himself.

The magnitude of this undertaking is set forth so capitally well in semi-popular language in his "Reminiscences," published in 1903, that it may best be indicated to you by quoting from this most interesting volume.*

Three years previous to the publication of his "Reminiscences," namely, in December, 1899, I had occasion, as retiring President of the American Mathematical Society, to review the same general field of work in an address on "The Century's Progress in Applied Mathematics." It was necessary in this review to indicate the state of astronomical science as it was left by the brilliant researches of Lagrange, Laplace, and Poisson at the time of the death of Laplace, in 1827, and to show what steps were necessary to further advancement. For the purposes of the present occasion I can do no better than to quote a few paragraphs from this address.†

Such, then, was the state of astronomical science about 1860, when Professor Newcomb began to consider seriously the unparalleled task he undertook to accomplish. The most

*See pp. 198-204 of "The Reminiscences of an Astronomer," by Simon Newcomb. Boston and New York: Houghton, Mifflin & Co., 1903.

†See pp. 139 and 148-151 of Bulletin of the American Mathematical Society, 2d Series, Vol. VI, No. 4, New York, January, 1900.

difficult of all the problems he had to attack, and the one to which he devoted the greater part of his life, is the capital gravitational problem presented in the motion of the moon. As has been indicated already, this motion had baffled the ingenuity of his most eminent predecessors and contemporaries, and, as shown in his most recent contributions to the subject, this motion is not yet fully harmonized with the Newtonian law. It should be said at once, however, that he made great progress in the treatment of this grand problem, and his tenacity of purpose as an investigator is well attested by the fact that his last as well as his first more important memoir is devoted to this uniquely difficult subject of research.

The first memoir, to which reference has just been made, was published in 1875, and is so important a contribution that it must serve as a test for all previous investigations as well as a foundation for all future researches in the subject. During the ensuing forty years Professor Newcomb published many minor papers and memoirs bearing on this subject, terminating with a summary which appeared in the monthly notices of the Royal Astronomical Society in 1894. Soon after he became a Research Associate of the Carnegie Institution of Washington, in 1902, he renewed his attacks on the subject and issued, in 1907, an additional memoir on the inequalities in the motion of the moon produced by the action of the planets.*

More recently he recurred to his earlier memoir and took up again a discussion of the mean motion and other elements of the moon as derived from eclipses and occultations from the period of the earliest Babylonian records to the present time. This was his last great work in this field, and it was his good fortune to announce its substantial completion on the occasion of my last conference with him, in the latter part of May of this year, when he was fully aware of the approaching termination of his activities. This memoir, I understand, is to be published, as are all of his principal papers with reference to the solar system, under the auspices

*Publication No. 72 of the Carnegie Institution of Washington.

of the American Ephemeris and Nautical Almanac Office, of which he was long the distinguished head.

As already indicated, the particular problems to which Professor Newcomb addressed himself forced him to make many investigations in closely allied fields of astronomical research. One of the most important of these, by reason of the fundamental rôle it plays in all astronomical work, is the preparation of catalogues, or ephemerides, giving precise positions of the so-called fixed stars. Indeed, it should be said that he was one of the first to introduce order into the various publications giving stellar positions, the principal of these publications being the British Ephemeris and Nautical Almanac, the American Ephemeris and Nautical Almanac, the *Connaissance des Temps*, and the *Berliner Astronomisches Jahrbuch*. A half century ago these publications were based on differing data and gave, accordingly, different results for the use of the astronomer and the navigator. In seeking to bring order out of this astronomical chaos Professor Newcomb performed a signal service to the world of astronomy and pointed the way to the great advances that will be made in the near future. The labor involved in collecting the data, in discussing the observations, in deriving trustworthy results, and in preparing the requisite tables for their use is such as few individuals are qualified to undertake.

While all these investigations to which reference has been made were going on, Professor Newcomb found time to publish numerous important contributions in almost every other field of astronomical investigation, and also to make many excursions into the various fields of pure mathematics, political economy, statistics, and even into what would appear to most minds to be for him the remote field of fiction.

This uncommon versatility helps to illustrate a very noteworthy characteristic in Professor Newcomb, namely, what may be called the plasticity of his mental makeup. His was one of those minds, none too common, which appear to continue to grow throughout the lives of their possessors. During the past twenty years I have had frequent occasion

to work with him on committees and to consider with him questions raised by a great variety of topics, especially those relating to the conduct of the work of scientific societies. During these years I have been repeatedly struck with his disposition and capacity to see the various sides of questions which came up for consideration. He was always ready to listen to an expression of opinion from his opponents, and he was always ready to anticipate the results of any measure proposed for action. He was one of the relatively few individuals I have met who always ask what will happen next if we adopt any pending measures for application in the conduct of a new enterprise.

During many conferences and talks and walks with him in recent years, I found him full of suggestions with regard to almost every topic which might arise for consideration, and as a subject of study the operations of his mind came to be of great interest to me. Something over a year before his death he promised to write out for me his own impressions with regard to the workings of his intellectual machinery, but, although keenly interested in this line of inquiry, it does not appear that he found time to comply with my request.

One of the striking characteristics of his mind, often misinterpreted for indifference or inattention, was due to his superior capacity for mental concentration and his frequent absorption in thought. This characteristic he defended in his "Reminiscences" and often in conversation. The possession of this trait in an unusual degree helps to account not only for his unrivaled productivity but also for the unusual clearness of his written work. He enjoyed the distinction amongst his fellow-countrymen of being able to write the clearest English produced by any man of science in America. An illustration of this degree of absorption may not be out of place here. During the last eight or ten years of his life he was continuously and by unanimous approval President of the Astronomical and Astrophysical Society of America. It happened on one occasion that the council of this society held a meeting in one of the high-school buildings of the city of St. Louis. At this meeting a considerable amount

of routine, but not very important, business was transacted, all without incidents which might arouse special attention. When the council adjourned Professor Newcomb arose from the presiding officer's chair, put on his overcoat and hat, and started to go out of the room. Immediately adjacent to him were the coat and the hat of another member of the council. These Professor Newcomb proceeded to lay carefully on his arm and carry out into the street, until he was followed by the owner and reminded that probably one overcoat and hat per man would suffice. While all this was going on, greatly to the amusement of his colleagues, his face assumed an attitude of complete abstraction, and his mental machinery was undoubtedly deeply absorbed with some intricate problem which had taken possession for the time being.

In his points of view and in his favorite pursuits Professor Newcomb was intensely practical. Although he understood the theories of pure mathematics uncommonly well, and occasionally made contributions to this domain of thought, he was far more deeply interested in the applications of analysis to physical science. In this respect he belongs to the school so well represented in Germany by Helmholtz and Kirchhoff and in Great Britain by Stokes and Kelvin. It would be a mistake, therefore, to rate him as a mathematician in the more recent sense of this term. He was rather a mathematical physicist whose work lay chiefly in the field of dynamical astronomy, and, as was the case with his eminent contemporaries just mentioned, success in his chosen field depended mainly on a masterful knowledge of both mathematics and physics.

This practical turn of mind was well shown also in many ways in the ordinary affairs of life and in his writings on finance and on political economy, as well as in certain projected investigations he did not live to carry out. His clearness of vision in all these matters shows that he might have been equally eminent in other fields than that of astronomy if he had devoted a like amount of attention to them. But, although he made many fruitful excursions into these other fields, his devotion to astronomy never faltered. Behind the

strongly utilitarian motives of his activities there was a high idealism which led him to hold with Laplace that—

“L’Astronomie, par la dignité de son objet et par la perfection de ses théories, est le plus beau monument de l’esprit humain, le titre le plus noble de son intelligence.”

ADDRESS OF MR. L. O. HOWARD,

PRESIDENT COSMOS CLUB; PERMANENT SECRETARY AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

The brief contribution which I shall make tonight to the memory of Simon Newcomb will be simply reminiscential in its nature, and will refer to only one or two sides of his many-sided and wonderful character. I knew him in the Council of the American Association for the Advancement of Science, on the Board of Management of the Cosmos Club, and as Executive Secretary of the International Congress of Arts and Sciences at St. Louis in 1903, I saw him intimately for a few weeks under extraordinary conditions which gave me an insight into his character which only those closest to him could have gained.

When I joined the Cosmos Club nearly five and twenty years ago, I was shown about by my sponsors who introduced me to this and that well-known scientific man, and who ultimately pointed out a sternlooking, bearded man engaged in playing chess, with the whispered comment “That is the most eminent member of the Club, Simon Newcomb, the astronomer and mathematician.” The reverential awe with which I watched him was thoroughly genuine and lasted for many years. His face seldom softened with a smile and, although presented to him several times, I never ventured to accost him. He seemed so aloof from ordinary men, and always in an attitude so superior to the small things of life, that in his presence I felt like one of the smallest of those things.

Years later I came into the Council of the American Association for the Advancement of Science, of which New-

comb, as a past President of that great organization, was a perpetual member. He had little to say, although his interest in the welfare of the Association was evidently deep, but when he did speak it was with a mixture of sound, scientific judgment and of clarified common sense that showed me that he was far more than the asbtracted scientific genius I had always supposed him to be. But he still seemed unapproachable. His unbending carriage, his leonine head, his stern expression, did not invite the approach of younger men. And so I knew him only from a distance, which, as I later found, was not to know him at all.

And then there came a summer (it was after his retirement from the active list in the Navy) when his wife was away and he was thrown upon the Club. The men of his own scientific pursuits and of his own time were no longer there; but Newcomb interested himself in the younger men—not only those engaged in scientific work, but others. And he took his luncheons, curiously enough, with a group of architects—Glenn Brown, Dessez, Fuller, Hornblower, Marshall, Poindexter—and jokingly denied a seat at his table to all but architects. And then I and all the rest of us learned to know him better, and soon the sense of awe passed away and a feeling very much like affection took its place.

Soon after there began a miniature revolution in the Club. New by-laws were adopted, emphasizing the intellectual features of the organization, and, as the most eminent exponent of that aspect of the Club life, he was made its President by practically a unanimous vote. In the meetings of the Board of Management he took the most serious interest, even in the most sordid details of club management, and showed himself a man of rare business judgment and capacity, while in all his relations with the other members he exhibited a charming courtesy at first and a warm friendliness later that showed us how much we had lost in not penetrating his apparent reserve at an earlier period.

But it was the 1903 International Congress of Arts and Sciences at St. Louis which opened my eyes most fully, and those of many others, to the all-around greatness of the man.

He had been chosen first among the organizers of the Congress and as its President. He and the two Vice-Presidents, Doctor Münsterberg and Doctor Small, were sent to Europe to secure the coöperation of the leading men of science and art in all branches of learning, and to gain their support to the Congress and their attendance. It is obvious that no better choice could have been made, for Newcomb was, of all the American men of science, the one best known to European investigators. He was received everywhere with all the respect which his eminent scientific standing demanded and which his irreproachable character deserved; and the success of his journey and that of his colleagues was surprising. He returned to this country and immediately began the organization of the Congress. He secured offices and a corps of helpers, and worked night and day. He saw the necessity for the appointment of an executive secretary of the Congress in a person accustomed to the handling of large meetings of scientific men, and it was upon his suggestion that the exposition authorities appointed the speaker to that position. Arriving at St. Louis, in the hot days of late July, little had been done locally. The confusion attending the opening of a large international exposition had hardly passed away. The necessity for large and competent meeting halls in great number had not been met, and there was room for hard work and organizing ability in the extreme. All emergencies were met; the Congress assembled; distinguished men of all branches of learning came from all parts of the world; Newcomb was the controlling factor; his opening address, devoted as it was to the broad subject of the evolution of the scientific investigator, was broad and profound and eloquent. I quote a paragraph from his closing words, to give you an idea of the felicity and eloquence of his diction:

“The assembling of such a body as now fills this hall was scarcely possible in any preceding generation, and is made possible now only through the agency of science itself. It differs from all preceding international meetings by the universality of its scope, which aims to include the whole of knowledge. It is also unique in that none but leaders have been sought out as members. It is unique in that so many

lands have delegated their choicest intellects to carry on its work. They come from the country to which our republic is indebted for a third of its territory, including the ground on which we stand; from the land which has taught us that the most scholarly devotion to the languages and learning of the cloistered past is compatible with leadership in the practical application of modern science to the arts of life; from the island whose language and literature have found a new field and a vigorous growth in this region; from the last seat of the holy Roman Empire; from the country which, remembering a monarch who made an astronomical observation at the Greenwich Observatory, has enthroned science in one of the highest places in its government; from the peninsula so learned that we have invited one of its scholars to come and tell us of our own language; from the land which gave birth to Leonardo, Galileo, Torricelli, Columbus, Volta—what an array of immortal names!—from the little republic of glorious history which, breeding men rugged as its eternal snow-peaks, has yet been the seat of scientific investigation since the day of the Bernoullis; from the land whose heroic dwellers did not hesitate to use the ocean itself to protect it against invaders, and which now makes us marvel at the amount of erudition compressed within its little area; from the nation across the Pacific, which, by half a century of unequalled progress in the arts of life, has made an important contribution to evolutionary science through demonstrating the falsity of the theory that the most ancient races are doomed to be left in the rear of the advancing age—in a word, from every great center of intellectual activity on the globe I see before me eminent representatives of that world-advance in knowledge which we have met to celebrate. May we not confidently hope that the discussions of such an assemblage will prove pregnant of a future for science which shall outshine even its brilliant past?"

This occasion, which I consider to be perhaps the culminating public function of Newcomb's career, was a fitting climax to a life of research. Here in bodily presence he appeared, as he had so long been in the minds of intellectual workers, a great leader in the world of thought.

ADDRESS OF MR. HERBERT PUTNAM,*

LIBRARIAN OF CONGRESS.

I have listened to the addresses of Mr. Updegraff and Dr. Woodward with an interest the more keen because they embodied estimates, by men of science, of Dr. Newcomb as a man of science; and have heard with satisfaction the tribute by Mr. Bryce, because it selected for emphasis personal traits which had independently impressed themselves upon me; for my own relations with Dr. Newcomb were of course not scientific, but purely personal. They existed particularly in connection with a company which from time to time has gathered with me at the Library for luncheon. During the past year and a half that company has been sadly depleted; for it has lost its three most striking figures: Ainsworth Spofford, Edward Everett Hale, and Simon Newcomb. A table at which these three were present was, indeed, uniquely impressive: the bent absorption of Dr. Spofford; the towering, crag-like vigor of Dr. Hale, at once flashing and sonorous; the steady massiveness of Dr. Newcomb; Dr. Spofford reminiscent of innumerable recorded things; Dr. Hale vividly personal; Dr. Newcomb weighty with meditation of the great truths of nature itself, of cosmic laws, of vast spaces, of inconceivable, yet calculated dimensions.

Such a company, including as it did men of different type and vocation, further diversified by occasional guests of type, vocation and experience still different, with no set program, and with the purpose only of agreeable intercourse, was calculated to bring out individual characteristics of mind and of manner more vividly than one homogeneous in kind and in pursuit.

It was as a member of this company, then, that I had particular opportunity to observe Doctor Newcomb. And the

*Mr. Putnam had accepted an invitation to participate in the meeting, but afterwards was compelled to withdraw because of contingent out-of-town engagements. As he was actually present, however, it is fitting to insert here the address he prepared for the occasion.

traits which he exhibited there would alone have sufficed to give me my admiration for him as a man of science and my affectionate regard for him as a man. The two were blended; for in him the scientist and the man were one. This unity was, indeed, I think, more notable in him and more attractive than in any one whom I have ever met. It brought the data of a remote science to the service of common converse; just as in him it brought the *method* of a remote science to the service of a mere political and social economy; and it seemed to augment the man into a reciprocal dimension with the subject.

As he would sit there at the table, with that Olympian head of his, superbly poised, there was in his very aspect a something nearly akin to the majestic; and the impression was confirmed by what Mr. Bryce has called the power and the insight of his eyes—a power, of course, consisting not in any physical singularity, but in the fact that they were the eyes, not merely of a close and keen observer, but of a great reflector. There was in them distance, depth, patience, labor, and repose. You felt the mind behind the eyes, and behind the mind you felt the immensities with which it was engaged. A demeanor notably quiet: not, for instance, like that of Doctor Hale, discursive and richly profuse. Compact rather, and reserved; yet more than responsive; and in its responses notably modest, appreciative, and informing. He once wrote of Agassiz that he was one of the three men (the others were Henry and Waite) who impressed [him] by a “certain artless dignity, which made their presence both instructive and charming as if they had never known what breeding was, because they had been born bred.” Such a dignity—distinctly artless—was his own. It was in part physical, sustained by a presence so imposing. But it would have signified little except for the mind back of it, and the nature also. It was not accompanied—nor enfeebled—by grace; rather the opposite. His gestures, for instance, in speaking to an audience, were distinctly shy, stiff and awkward; but where in this they failed to supply grace, they seemed to add impressiveness; for they suggested the elemental in the nature of

which they were the expression: a thought—or motive—beyond—not beneath—the capacity of his members. Elemental his nature was,—notably so,—its effect enhanced by lovable contrasts: a naïveté against his outer austerity, like sunny verdure against rugged cliff. Not himself either wit or humorist, he valued wit and had the sense for humor, and the keenest relish in paradox and pleasantry—a relish which subsisted seriously beyond the occasion.

I have heard that he was at times brusque of speech and inconsiderate of the feelings of others. Doubtless he was, or rather *seemed* so, especially in his earlier years, and certainly in his prompt contempt of anything which seemed to him chicanery. My own acquaintance with him was limited to the mellower period of his later life, and occasions in themselves mellowing. In memory, moreover, I idealize him. But that is precisely the privilege of this occasion, and the duty, for to idealize is to reach for and draw out the essential idea behind the external man; to seek the motive behind the manner, the character behind the characteristic. Of a breeding merely social, the breeding which imports refinement of speech and of manner, he had little. He may not have thought this worth while, or he may have been physically incapable of it. The shyness which, I believe, in him, as in many other such natures, accompanied his simplicity and directness, impeded. As with many others, too, it may often have given an impression less kind than he intended. So also doubtless did the absorption of mind, which in him, as in Dr. Spofford, produced an abstraction of manner likely to be misconstrued as indifference. On no occasion under my observation, however, have I seen his geniality waver, nor his tolerance before contrary opinion, nor his modesty in stating his own opinion upon matters within his especial field, nor his deference in relation to fields of which he had made no study. For the extent and precision of his field of knowledge was equalled by the candor of his field of ignorance: and yet there were very few fields, even of human affairs, to which he had not given some thought; for, like the astronomer Houzeau, his

reading and meditation included not merely astronomy, but meteorology, geography, geodesy, philosophy, literature, political economy; and, like the great Belgian, his object in life was to help in the cause of humanity as well as of science.

He was never, I say, intolerant against contrary opinion which he supposed sincere; yet, a master of an exact science, he required of others that exactitude in expression which he imposed upon himself. While therefore patient before assertions of fact or of principle opposed to his own impressions or convictions, I have often remarked his insistence upon a clear expression of them, and have as generally observed that the inability of the interlocutor to frame such an expression concluded the argument. In an address as a former President of this Society, after asserting "the greatest want of the day to be the more general introduction of the scientific method and the scientific spirit into the discussion of our political and social problems," he explained that under the scientific method must be included discipline in the scientific use of language. To such a discipline he had doubtless subjected himself. Indeed, there was evidence that he was doing so daily. The result was a style, both written and spoken, of extraordinary lucidity. He has been called "a master of clear thought and of good English," but he was more than a master of good English: he was a master of clear expression, and he could have been a master of clear expression only *because* he was a master of clear thought. His capacity for simplicity of expression amounted to genius, and, when it was sustained in the exposition of even the profounder problems of astronomy, you felt not merely that the conclusion was convincing, but that the genius of thought lay back of it. The intricate formulæ with which he was engaged, which would have rendered a little man confused, seemed in this large man but to confirm his fundamental simplicity.

To Dr. Newcomb the universe was either intelligible or unknown. If intelligible, it must be capable of clear and exact expression; if unknown, it must be clearly distinguished as such. With the enthusiasm and capacity for

speculation which, together with patience and reserve, Dr. Eliot declares the necessary equipment of the scientific investigator, he thus never failed to distinguish mere hypothesis from a conclusion reached by induction. Nor did he think it necessary to the ascertained conclusion that it should be stated dogmatically or otherwise than with moderation.

If this general attitude of Dr. Newcomb was remarkable for its lack of pretence, his attitude toward his own specialty was still more so—I mean in its modesty with reference to his own achievements, his generosity toward those of others. These traits are strikingly evidenced in his "Reminiscences," a book charming as literature, but also, to those who cared for the man, most winning in revealing him. Written toward the close of his career, when his own great work was before the world and his fame established, these offered the opportunity, if vanity craved it, for pardonable complacency. They offered also the opportunity, which a lesser nature could not have resisted, for that parsimonious praise of others whose effect is depreciation.

But so far removed are they from parsimony that his references to the abilities and accomplishments of other astronomers are as hearty and even respectful as if he were a novice in the science writing of the masters. Mr. (Geo. W.) Hill, for instance, he records as "the greatest master of mathematical astronomy during the last quarter of the 19th century," who, in the determination of certain actual quantities, "has left every investigator of recent or present time far in the rear"; a tribute the more striking (and unusual) because Mr. Hill was an associate and a subordinate. The chapter in which it occurs is entitled "The Author's Scientific Work," yet it is a chapter largely devoted to the recognition of that of others. Airy, the Astronomer Royal of England, was "the most commanding figure in the astronomy of our time"; Auwers, "the highest type of the scientific investigator of our time"; Hansen, "the greatest master of celestial mechanics since Laplace"; while Delaunay's investigation of the moon's motion was "one of the most extraordinary pieces of mathematical work ever turned out by a single person."

And these appreciations were from a man himself now characterized as "the greatest astronomer of his time," "at the head of the cultivators of the astronomy of appreciation," whose own particular field was celestial mechanics, and whose task (to build up the whole theory of our planetary world on an absolutely homogeneous basis of constants) has been declared "almost superhuman"; a man who was in fact one of the gigantic figures in science generally, "worthy to be ranked with the rarely great, like Bichat in medicine, Archimedes in mechanics, and Darwin in evolutionary thought."

The essentials of his achievements in astronomy included, of course, a definite and particular purpose and a prodigious capacity for mere labor. They necessitated absorption and concentration; but for a man so engaged upon a field so remote he had a singular interest in and touch with matters near at hand. The affairs of the stars never wholly abstracted him, any more than they did Houzeau, from the affairs of earth, and his certainty as to the regulation of the former by law induced him to believe in the existence of laws among the latter which could be ascertained and should be regarded. In his political economy he enunciated at least one such law, whose influence upon economic thought may be lasting. I do not know that the treatise as a whole is a contribution likely to be, but that for its time and within its scope it was clear and helpful in its exposition and essentially sound in its tenets was, I believe, its general reputation. He had time for other excursions, even into the field of romance; but, as Sainte Beuve remarks in his essay on Franklin, such excursions or speculations are but the recreation of the man of exact science. The theme of that romance, by the way, was, as you will recall, a discovery by which the effect of gravitation could be overcome. In that very essay of Sainte Beuve's there is quoted a letter from Franklin to Priestley, written in 1780. In this letter Franklin regrets having been born so early, imagining "the degree to which the power of man over matter may be carried in a thousand years." "Possibly," he adds, "man will have learned how to disengage from great bodies their weight, and give them an absolute lightness which will facilitate transportation."

I suppose that the writing of this romance, as well as his investigations in psychic phenomena, gave to Dr. Newcomb both pleasure and rest. I never saw that he was vain of such contributions, as men are apt to be with a vocation of great significance and avocations of slight—the writer of prose, with his occasional poem, for instance. But, then, I never saw in Dr. Newcomb any symptom of vanity whatever. Furthest removed from it was his naïvely grateful appreciation of appreciation, especially the warm, direct, and unaffected word.

These diversions, after all, formed but a slight fraction of his great employment, and this latter was above all remarkable for its concentration and the unity of its aim. If “happiness consists in the conscious pursuit of a consistent purpose,” his scientific life had within itself the certainty of happiness without any need of diversion.

It must have had also the incessant uplift of the subject itself. To be on easy terms with the stars!—There is no hierarchy among the sciences; there is, I suppose, no science which is less than ennobling; but in astronomy there must, to a mind sufficiently large, be something still *enlarging*—the magnitude of the dimensions involved, the aspect of the universal and persistent reign of law, the subordination of the lesser to the greater, the very background of immensity, depth beyond depth, splendor above splendor. To Carlyle, who had somewhat studied astronomy, the perception of these things was, if not disquieting, neither calming nor orderly. With Newcomb the effect of the profounder studies, completely scientific, was distinctly so. “Carlyle” (remarks John Burroughs), “seemed always to see man and human life in their sidereal relations.” So, no doubt, did Newcomb, but in the reign of law Carlyle saw also the *tyranny* of magnitude, and beside and beyond the depth and the splendor he saw “terror upon terror.” To Newcomb the reign of law was universal, but for that very reason cause of assurance, and his calmness before the great truths of nature which came within his ken was coupled with a faith in the benignity of those which did not.

No doubt this faith sustained him in the physical trials which during his latter years he had to undergo, among them the malady in his leg, which for months entailed acute suffering and which, not yielding even to an operation, was finally only *bullied* into submission. I use the term of the prescription itself, but only those who have tried such treatment for such a malady can estimate the heroism which it implied. He undertook it and carried it to success, un murmuring, so far as we could see, and even humorously.

And that faith must peculiarly have sustained him to the triumphant heroism of his last days, when, knowing his end near, his opportunity for service ebbing, he persisted still in giving the finishing touches to the work which was his last great contribution to science. For, to the several traits which alone of many I must content myself with emphasizing—to the spacious yet consistent mind, to the deep yet clear understanding, to the patient will, to the simple, modest, lucid, affectionate nature—to all these add one which comprehended and amplified all—Sincerity. I mean that word and that trait to the full measure of it, and, as it was not diminished in him, so I will not diminish it by either elaboration or paraphrase. Its expression was in every personal relation of which I saw evidence, but it was also, of course, profoundly in his work itself: sincerity, and loyalty. At its close he might surely say, with Sir John Herschel:

“To thee, fair Science, long and early loved,
Hath been of old my open homage paid;
Nor false, nor recreant have I ever proved,
Nor grudged the gift upon thy altar laid.”

ADDRESS OF MR. E. M. GALLAUDET,
PRESIDENT OF THE COLUMBIA INSTITUTION FOR THE DEAF AND DUMB.

Professor Newcomb, already interested in mathematical and astronomical studies, came several times to Washington at the age of eighteen to borrow books from the Smithsonian Institution. He was then teaching in a public school in Prince George County, Maryland.

These visits to Washington gave him the opportunity of making the acquaintance of men prominent in the scientific world, notably Professor Henry, of the Smithsonian Institution, and Professor Hilgard, of the Coast Survey. These men became warmly his friends and encouraged and aided him in his studies.

At the age of 26 young Newcomb was appointed to a professorship in the Navy, assigned to duty in the Naval observatory, and gave most of his time to the observation of planets. Night duty proving unfavorable to his health and the condition of his eyes, he took up the line of mathematical work which he followed with such brilliant success all through his life.

In 1863, at the age of 28, he married Mary Caroline Hassler, a daughter of Dr. Hassler, a surgeon in the Navy, who lost his life in the wreck of the steamer *Atlantic* in Long Island Sound. Miss Hassler's grandfather was Ferdinand Rudolph Hassler, founder and first Superintendent of the United States Coast Survey. Miss Hassler's maternal grandfather was Michael Nourse, long-time chief clerk in the office of the Register of the Treasury.

For a few years after their marriage Professor and Mrs. Newcomb lived with Mrs. Nourse on 13th street near Pennsylvania avenue. In 1869 they established a home of their own on 11th street near N.

The social side of Professor Newcomb's nature was strongly marked. That is to say, he was fond of social life when it brought him in contact with intelligent and agreeable people. For fashionable society he cared nothing.

As soon as he had a home of his own he showed his disposition to entertain his friends in sensible ways. There were a number of young unmarried men in the Naval service with whom he often came in contact, and he established a custom of having several of these at supper on Sunday evenings, with a musical hour afterwards, singing hymns, which he was fond of hearing, though not himself a singer.

All through his life he was a hospitable host to men of science visiting Washington and to scientific organizations holding meetings here.

When Professor Langley entered upon his duties as Secretary of the Smithsonian Institution, Professor Newcomb gave a reception in his honor, and some here present will remember a brilliant dinner he gave in honor of a distinguished diplomat, which brought together more than sixty of the scientific and literary men of the Capital.

During many years he kept up a custom of inviting a number of friends to dinner on his birthdays. On his 60th and 62d birthdays, the last marking his retirement from active service, his house was hospitably open to his many friends.

That Professor Newcomb's sentiments and practice in discharging the duties of hospitality had been accepted and followed in the Cosmos Club, of which he was an early member, and later the president, is shown in an address he delivered on the twenty-fifth anniversary of the foundation of the Club when he said:

"There are some features of our organization which are not only distinctive, but almost unique. I believe that almost every other club, here or elsewhere, has no other important purpose than that of affording facilities for the social reunion of its members, and for promoting some idea of common interest to them. But we had what almost might be called a philanthropic object from the start. Our membership has been, from the beginning, largely composed of members of the various scientific societies of Washington. These societies had keenly felt the need of a place of meeting. Such a need the Club supplied from the time when its funds permitted it to join to our house an assembly hall. But we did not stop there. We all considered it not only

a pleasure, but almost a duty, to welcome to our midst the representatives of science, learning, and art from our own or other countries. Thus every national society holding a meeting in Washington finds the facilities of the Club placed at its disposal, not only for the individual uses of its members, but, when our modest assembly hall will answer the purpose, for the use of the visiting society. We have not allowed any learned association coming here for a meeting, or any man of eminence in the learned world to visit the city, without, if the case was known to our members, tendering our hospitalities. We thus aim to show to the world at large what Washington is trying to be and to do."

When sojourning in foreign lands Professor Newcomb received many social courtesies which were very agreeable to him. He had the honor of being entertained at luncheon by the Emperor of Germany, and was received at court by the kings of Italy, Sweden, and England, and by the President of France.

Professor Newcomb never cared for games of cards with others, but was fond of solitaire, and of working out some puzzles with cards.

He was devoted to chess and often played a game without having the board before him, carrying the position of each man in his mind as the moves went forward.

Professor Newcomb was most loyal in his friendships. Many illustrations of this could be given did time allow. One will suffice.

In 1869 he was observing an eclipse in Des Moines, Iowa, and was there taken severely ill. A young resident of the city cared for him as for a brother, bringing him all the way to Washington on a bed. The friendship thus begun continued during the forty remaining years of Professor Newcomb's life, not only in Washington, where the friend came to occupy positions of prominence, but in foreign capitals, where he represented our government in royal courts.

Professor Newcomb had decided views in regard to religion. He never united with any church, but made it a practice during many years to attend services with his wife. He maintained the custom through life of saying grace at table, and often gathered his children together after dinner

for the reading of a passage from the Bible and to repeat the Lord's prayer. He looked forward with confidence to a future life, and not long before his death spoke of his anticipated pleasure in meeting those he had never seen on earth, but whose characters he had admired.

In his "Side-lights on Astronomy," speaking of the work of an astronomer who died long before he was born, Professor Newcomb says:

"He was a lover of science and an indefatigable worker, and he did what in him lay to advance our knowledge of the stars. I love to fancy that in some other sphere, either within this universe of ours or outside of it, all who have successfully done this may some time gather and exchange greetings. Should this come about there will be a few—Hipparchus and Ptolemy, Copernicus and Newton, Gallileo and Herschel—to be surrounded by admiring crowds. But these men will have as warm a grasp and as kind a word for the humblest of their followers, who has merely discovered a comet or catalogued a nebula, as for the more brilliant of their brethren."

Professor Newcomb loved poetry and memorized it easily. Often when driving with his wife he would recite long poems which he had read years before. He was devoted to the works of Milton and especially fond of the "Hymn on the Nativity."

Addison's ode, "The Spacious Firmament on High," was a favorite he often recited, dwelling with emphatic earnestness on the closing lines:

"In reason's ear they all rejoice,
And utter forth a glorious voice,
Forever singing as they shine,
The hand that made us is divine."

We are here this evening to do honor to the memory of one who in the language of common usage is said to have died. May we not assure ourselves that this is only a small part of the truth concerning our friend. That part of him which died rests in the sacred ground at Arlington. But that other part, which for so many years communed with

the starry heavens, he had faith to believe would pass through the portal death to the life which has no ending.

May we not share his belief and that of a member of his family who, while on the train journeying to Washington for her father's funeral, wrote the following lines, which were sent to the Honorable John A. Kasson :

Oh, Father God, our Fountain Head of love,
Take back this child of Thine into Thy other home—
This soul, whose clear and piercing eye did gaze
Always upon the starry space above.

A child? Yes; for that clear and piercing eye,
Which glanced at mysteries veiled to other men,
Was the expression of a spirit meek,
Reverent, and full of awe for all on high.

It is not always with such awe as this
The eager eyes of searchers after truth
Seek for an answer to the endless why
Which questions them from starry heights or deep abyss.

It is not always in these modern days
We hear the words he most loved to repeat,
"The spacious firmament on high proclaims
Its great Creator and His wondrous ways."

God grant his character may with us dwell—
Faith, perseverance, patience, love of truth;
Then, when the call comes, "Cease thy work,"
Our answer may be also, "it is well."

RESOLUTIONS PRESENTED BY MR. O. H. TITTMANN,

SUPERINTENDENT, COAST AND GEODETIC SURVEY,

Adopted by the Society at its meeting of December 4, 1909.

WHEREAS the Philosophical Society of Washington has been deprived by death of the fellowship of Simon Newcomb, and

WHEREAS he was for thirty-eight years one of its active members and twice served as its President, be it

Resolved, That the Society record its high appreciation of his phenomenal talents, his pre-eminent attainments and his scholarly discussion of the many topics which his broad sympathies and varied interests proposed for consideration.

And be it further

Resolved, That this Society unite with the learned societies and institutions of the entire world in testifying to the loss to science and high learning which his death occasioned; and that we hereby convey to the bereaved family assurance of our profound sympathy.

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MUSIC AND SCIENCE

ADDRESS OF THE RETIRING PRESIDENT

CHARLES KASSON WEAD

READ BEFORE THE PHILOSOPHICAL SOCIETY OF WASHINGTON,
JANUARY 15, 1910

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MUSIC AND SCIENCE.

BY

CHARLES KASSON WEAD.

(Address of the retiring President, delivered before the Society,
Saturday evening, January 15, 1910.)

We meet tonight under unusual conditions. About a year ago the Society elected as President its most distinguished member, Simon Newcomb; after a long and painful illness he died July 11. In December, on the day fixed in the by-laws for the annual address of the President, we listened to tributes of esteem and to able addresses commemorating his life and services. To attempt to add anything tonight to what has been so appreciatively said by men who knew him and his work so well would be superfluous and inappropriate.

To the office left vacant I was elected for the remainder of the year; and members of the Society, jealous of its traditions, insisted that one who had held the honor, if only for a short time, should fulfill, at the earliest practicable date, the duty of presenting a Presidential address.

The subject I have chosen belongs in part to one branch of Physics: while this branch receives the smallest number of pages in a text-book, has the fewest votaries, and lies at one side of the modern current of spectacular or industrial research, there is, on the other hand, no branch of physical science which deals with subject-matter lying so close to the heart of humanity as Acoustics. In studying electricity or thermodynamics, who ever thinks of man's desires and actions as part of the material to be investigated? But Acoustics belongs to the humanities as well as to mathematics

and physics; and men's actions, likes and dislikes, as expressed in music, furnish a large part of its most interesting subject-matter. Unfortunately the musicians, ethnologists and physicists sometimes appear to be more ready to dispute than to coöperate in studying the subject. So the most desirable thing at present is that all who are interested in the matter shall cease to say—You ought or ought not to do so and so,—and shall seek first to learn all the actual facts relating to the past and present of music, and then to consider their significance. Before long the histories and philosophies of music must be rewritten because of the many facts not at the command of older writers. Let us this evening try to get a bird's-eye view of the subject in some of its broad aspects.

To this end, suppose that a superhuman being from Mars should cease digging new canals for a time and should visit our earth, alert, intent on seeing every form of human activity and learning the chief conclusions of human inquiry and research, unprejudiced, and able to consider men's actions, thoughts and feelings as objectively as he would rocks and animals; and suppose that we may accompany him as he hastens over the world.

Our visitant would soon learn that music is one of the great human activities;—he learns that in our country the census classes 100,000 persons as "Musicians," and puts the annual production of instruments at \$100,000,000; he sees our children spend many hours a week in the study of music; often he sees thousands attending a concert or opera where a hundred persons are busy making sounds; he finds the tumult of sound is all "a mighty maze, but not without a plan."

To learn something of this plan he examines instruments like those in the fine collections at the National Museum, or the New York Metropolitan Museum, consults histories and theoretical books, and asks how music has developed, what its physical, physiological and psychological foundations are, and what music means to those who compose, perform, and listen to it. Unfortunately human students of these things cannot yet answer the questions fully and satisfactorily; but some results already gained deserve attention.

One recognizes early that while musical activity is found among almost all peoples, throughout all history, the specific form in which it finds expression has been and is very varied, and what gives satisfaction to one people is often unpleasant to another; yet one finds uncritical men assembling in magazine articles comments "on music" from writers in the most diverse conditions; although, perhaps, no two writers had the same concrete idea back of the word. For in reality the idea of music is not only as generic and indefinite as that of tree or animal, but it is constantly being extended to cover new forms. A good modern definition is that of the Oxford Dictionary:

"Music. That one of the fine arts which is concerned with the combination of sounds with a view to beauty of form and the expression of emotion; also the science of the laws or principles (of melody, harmony, rhythm, etc.) by which the art is regulated. It has two branches, the art of the composer and that of the executant."

Following the order of ideas in this definition, one notes, first, that the "fine arts" are primarily to be enjoyed; their educational relations should be held secondary. It ought to be obvious that a strange thing cannot be understood or enjoyed at once; familiarity with it or with closely allied things is indispensable; but the contemptuous remarks of travelers about Arab, Hindu or Indian music are far more easily found than the appreciations of those who have learned to enjoy it. Besides the pleasure that comes from suitable bodily activity and coöperation with others, as in choral singing, the elements of musical enjoyment are partly material, sensuous; and partly psychical, mental. To the first belong the beauty of tone, as in the voice; the contrasts in timbre or quality of orchestral instruments; the variations in pitch and loudness of the notes of a melody; and most important of all, rhythm in its infinite variety; primitive and Oriental rhythms are far more complex than those of European musicians.

The principal mental elements of musical enjoyment depend on memory, and may be included under the compre-

hensive term, association; "Yankee Doodle" heard in a foreign land, the "Marseillaise" heard anywhere, the memories recalled by a strain heard long ago, illustrate the sentimental phase of this; all songs, hymns, operatic airs necessarily have association with ideas expressible in words, though oftentimes it is arbitrary and unnatural; even music without words often has a sort of program or label with it, due to the composer or critic, as the "Pastoral Symphony." Another kind of association of sounds is that involved in such terms as "scale," "harmony," "tonality;" which will be considered later. But no scientific analysis of music into its elements pretends to explain fully its charm or power; for in every work of art the whole is more than the sum of its parts, as a house is more than bricks and lumber.

The next idea in the definition is "combination of sounds." The sounds may be successive, one at a time, the result being a simple melody or tune; if all the sounds used in it are re-arranged so as to come in order of pitch, the result is a *scale* (a more exact meaning of this word will be given later). Or the sounds may be simultaneous; the result, often loosely called harmony, may be (1) two or more parallel lines of melody, as when men and women sing the same air in Octaves, or when men in medieval times, or primitive singers today, take parts at intervals of a Fourth or Fifth; (2) two or more nearly identical parts, but begun at different times, as in a fugue or catch; (3) two or more independent tunes adapted to one another, as in the old polyphony; (4) true harmony, where the successive elements are chords or discords, not single tones. The evidence is very slight that non-European peoples have ever known any kind of harmony except the first.

All these sounds are to be combined with a view to "beauty of form." Musical form is as varied as poetical form, and the fashion changes from generation to generation; one does not look often for fugues or sonatas or rhapsodies from recent composers. Each age has its rules by which to measure music and a musician, as Wagner found to his sorrow, and illustrates in his "Meistersänger." The singer who took the part of Venus in the first performance of Tannhäuser in 1845 said

to him, "You write such eccentric stuff it is hardly possible to sing it." But in all generations, like a poem or a drama, every musical work of art must have movement without complete pause till the end is reached; and certainly in the past the best music has had "unity in variety." Sometimes this has been bluntly forced on one's attention, as by the old-fashioned title "Theme with Variations;" generally it is more subtle; the school girl had a true sense of this principle who said of "In Memoriam," "It's wonderful in how many ways Tennyson says the same thing." Variety is lacking in trumpet notes and generally in primitive songs and tunes, and however enjoyable they are at first the listener soon tires of them, as one does of repetitions fit only for children. At the other extreme, one may find it difficult to see the unity, to keep the connection, in a movement of a sonata, as in a passage of Henry James or of Robert Browning; obviously the intelligibility of these depends always on the training and often on the mood of the reader; and obviously, too, a pianist by proper phrasing, or an elocutionist by the voice, can make clearer an involved passage; it is at this point that automatic piano-players are most likely to be found wanting.

The paramount unifying principle is named Tonicity. This means that out of the ten thousand tones of different pitch that our ears can discriminate, even out of the twelve tones in an octave on the piano there shall be used in any short passage only a few, which stand in definite, easily recognized relations to one or all of the others, and when other tones are wanted they shall be introduced in a recognized way; too much freedom would lead to chaos; too little, to monotony. The names of some of these relationships are: Tonic, Tonic Chord, Dominant, Mediant, Leading Tone, Third, Major, Minor, etc. If there were time, many curious quotations might be made regarding some of these terms; many men, even Goethe, appear to have been almost bewitched by the word "Minor." It may contribute to accurate thinking to notice that the clearest, most satisfying relation is generally recognized to be that of the major tonic chord in its first position, as C, E, G; and these sounds are heard as a broken

chord in successive notes from a horn or bugle. Other chords are less clear, joyous, self-reliant, inspiring; some are minor chords or even discordant; popularly any chord or passage of music not clearly major is loosely called minor. The older rules required that the key or tonic of a composition should at once be made clear, and that after modulations into other keys the piece should end in the original key and in a conventional way; the absence of a regular cadence is a striking feature of much primitive and of some modern music. All these views of beauty, form, and unity belong to the esthetic theories of a generation or two ago; whether the confessedly ugly but powerful things, which modern musicians, painters and sculptors are furnishing, shall ultimately be accepted must be left to the students of esthetics a generation hence.

In connection with the subject of form and its elementary structural material, the scale, it is important for the physicist to realize that when Helmholtz wrote his great work, "The Sensations of Tone as the Physiological Basis of Music," about forty years ago, these views were prevalent; the sonata-form still dominated musical thought, and the scale furnished by the piano was thought of in diatonic or harmonic terms; modern composers have cut loose from most of the classical traditions and rules; and how much of Helmholtz's musical discussion is applicable to modern work, probably no one has attempted to say; the purely physical investigations of course are still valid, though some important additions have been made to them by later experimenters. Further, it must not be imagined that unity is dependent on a sense of tonality, for the *Ethos* of Greek music, and the Hindu *Rags*, involve other principles of unity not yet made clear; and if tonality is to be predicated of, say, Arab music the word must involve mainly or wholly melodic relationships, not harmonic.

The remaining term in the definition is "the expression of emotion." At all periods there have been attempts at realism in music, by imitating or suggesting sounds in nature, the movements of a horse, birds' songs, the noise of waves, battle-scenes, etc., but these form only a very small part of musical literature. There have been frequent attempts, too, to put

into words the thought expressed in a purely instrumental piece of music; but the "expressiveness of music," in this sense, generally eludes precise statement, and if stated, the conclusions of one generation afford laughter for the next; (as the comments on a Beethoven symphony quoted in a recent concert program). Most of such writing is futile, for the deeper ideas properly expressible by sounds, or colors, or words, or forms in sculpture have little in common and cannot well be translated into another language. The artist, "the man who is master of his materials," can rarely use well two different media of expression. But music, when spontaneous, expresses the emotion or feeling of its author, and even when artificial may voice a mood or may project before the mind large, indefinite ideas, and so it will inspire in its hearers the same emotion or mood to some degree. Naturally songs furnish numerous instances of this truth, for the words and music coöperate, while many legends of the power of music attest it; and even the most unresponsive of us has sometimes felt the spell of a purely instrumental composition, performed by one in sympathy with it. Perhaps the most striking illustrations of the emotional expressiveness of music are to be found in the modern operas where the music is to assist the words and gestures of the singer; Wagner furnishes many such cases, which critics have often cited; Redfern Mason, in the "*Atlantic*" for January, 1910, cites later instances:

"When Strauss wants to convey the idea of the weakness and depravity of Herod, he does so partly by halting, vacillating rhythms, partly by a theme based on a scale of whole tones. . . . C, D, E, F[♯], G[♯], A[♯], C is the scale used by the composer of 'Salome' when he wants to portray degeneracy. It jars on the ear, but not more so than the character of Herod on the mind." "Strauss boldly realizes in music the ineuphoniousness of sin." . . . "He deepens the moral significance of music; . . . the music of John the Baptist is gravely diatonic; of Salome, chromatic; of Herod, abnormal." Debussy also makes use of abnormal aspects of

tonality, as the church modes and a scale of whole tones, as the succession B, A, G, F in "*Pelléas and Mélisande*." Much of the music of both Strauss and Debussy is in no determinate key—sometimes in two keys at once—e. g., "voice and orchestra may be in different keys to express doubt or consternation." As has been said of Wagner's novelties, these resources "are possible to the composer only because the hearer's sense of tonality has become so highly developed."

But enough of definitions and comments on them: let us go with the inquiring visitant to learn of more primitive music. In many countries of Europe there has been much activity in rather recent years in collecting folk-songs from the mouths of singers; some good critical work along this line has been done by the English Folk-Song Society, and its secretary, Mr. C. J. Sharp, has felt warranted in publishing a book entitled "*English Folk-Songs—Some Conclusions*." In distinction from art-song, which is the work of one man at one time, and which other singers may repeat with scrupulous accuracy, a folk-song begins, no one knows where, goes from mouth to mouth, being unconsciously modified many times; never reaching a finished state, but always existing in several forms, and yet being a composite expression of the esthetic sense of the community on this little subject; it is communal, the product of a race. We all know that a negro melody like "*Swing low, sweet chariot*," is in a different class, every way, from Foster's and other imitation darky songs. The English folk-songs when analyzed show to Mr. Sharp a marked absence of harmonic relationships; they are more like the medieval church-tones, having often a scale from D to d, or G to g, etc., rather than from C to c; they are rarely accompanied and cannot be harmonized without sacrificing their charm. Most of the older collections of alleged folk-songs are said to be uncritical and untrustworthy.

In a higher state of society the visitor finds instruments and musical notation; the piano keyboard and the complete staff-notation are some 200 years old, the results of earlier centuries of development; but except in the earliest times

they have been almost ludicrously inconsistent with the musical theory of their time, and have never been consistent with one another. The musical interval between notes on two adjacent lines is sometimes a major third and sometimes a minor one; on the staff 8 lines and 7 spaces span two octaves; on the keyboard 8 white keys span one octave; the white keys of the keyboard give the diatonic major scale of C and this only, although since Bach the theorist and the tuner have maintained the equality of the twelve keys; so the student has to learn 12 ways of fingering each chord, instead of one only, as with the practical Janko-keyboard, two of which have been owned here in Washington. As for notation, disregarding the trivial proposals for colored notes and for heads of various shapes and schemes for some particular instrument, analogous to the old lute-tablature, only two plans for improvement deserve notice: one, to use 12 lines to the octave, a plan not approved by musicians, but extremely convenient for the scientific student of scales of Oriental peoples and of non-diatonic music; the other, the use of the initials of the syllables, do, re, mi, etc.; such music can be set up in any printing office, and the system, known as the Tonic-Sol-Fa system, has been so perfected that in England there is a great amount of music, new and old, available in this notation and it has contributed much to the success of the great choral societies and their festivals. In the excellent text-book on physics of Jude and Gossin this notation is used.

Turning to instruments, a comparison of those in present use with those in museums shows how the soft-toned older ones have been driven out by those more powerful. The iron-frame piano, invented in this country and severely condemned by European musicians in the Exposition reports of 40 or 50 years ago for its noisy unmusical tone, has triumphed everywhere, and is now still louder than of old, though of more even and perhaps better tone-quality. Recent improvement in the organ has been in the way of pneumatic and electric actions, and especially in the construction and voicing of pipes. In smaller instruments it has been mainly

in securing a more uniform tone-quality and in providing for more rapid playing; when a clarinet player must control 19 or more holes with only 10 fingers, he has need of all the assistance the skilled mechanician can give him. The student is struck with the fact that the practical instrument-makers seem to have made little use of the solid knowledge of the physicist, while some of the attempts to apply scientific principles have been musical or commercial failures; it seems to be thought that the popular books tell all that is known of the laws of vibrating bodies, whereas the statements in them are only first approximations. But the physicist must admire the marvelous skill of some makers and voicers of instruments and wish his apparatus were as reliable as the expert's ear in judging of quality of tone. The newest large instrument is the *choracelo*, exhibited last spring at Symphony Hall in Boston, a piano having both hammers and electrical devices for sustaining the tones and giving much change in quality.

If our visitant should inquire whether music has any moral qualities or effects (entirely independent of words) who could answer him confidently? The Greeks thought it had; the Chinese have insisted for many centuries that it has; of writers in English but one man is recalled, who seemed sure,—the Irish classical scholar, Mahaffy; he held that a passage of music may be as immoral as a picture or a passage in a book. (The influence of songs in school and home and church, and of music in medical treatment are different questions.)

And lastly, before the visitor leaves, let him tell how he finds music is regarded in the higher institutions of learning. In German universities only a few years ago its teachers were listed with those of fencing, dancing, and gymnastics. But now in several universities in Germany, England, and America there is a professor of music; how far the subject is taught (or can profitably be taught), not servilely or merely as a career, but as a branch of a liberal education, and with sympathetic insight into the musical activities of far-away peoples, it is not easy to learn; some men seem to be teaching

in this spirit; but in one university the professor of music is reported to have resented any scientific study of music as a profanation; and in another, the chair was endowed that its occupant might compose, not teach.

Having in such a journey with a superhuman visitor quickly collected a variety of facts about music, a few of which have just been recited, the scientific man may attempt to arrange and classify them, to analyze and resynthesize them, if he thinks it worth while; but before there was any modern science, this attempt was begun; the search for the basis of music antedates the search for the North Pole, the Holy Grail, the Philosopher's Stone. It will be enough for this evening if we consider some methods and results of investigation on the formation of scales.

For clearness, first recall our own piano-scale; it is intended to give twelve exactly-equal chromatic steps to the Octave; but the succession of the white keys is more often thought of, and this is nearly a diatonic succession, of seven unequal steps. This scale has to serve for harmony and for accompanied melody, and generally other instruments and the trained voice correspond to it. But it is sometimes deviated from intentionally; a solo violinist will sometimes make his Fourths and Sevenths sharper than the piano, according to his feeling in the particular piece he is playing; the violinist or singer will slide from one note to another; and the notes of a good string or trombone quartette do not quite belong to the piano-scale.

In contrast to this, note the Siamese scale, which has, according to their theorists, seven *equal* steps to the Octave; confirmation comes in two ways; first, by examining the pitch of instruments tuned by native musicians, and second by submitting to their judgment instruments tuned more precisely than they can do it. In China the old theory calls for a scale made by ascending Fifths, all the notes being reduced within one Octave: thus C, G, D, A, E; the next term in the series, B, is so close to C that unless the tuning were good the two notes might be so nearly the same as to be practically

duplicates and one of them useless; so a five-tone scale results; but the series is sometimes continued to give a seven-tone scale; our tuners complete the cycle to get a twelve-tone scale; and the Arabs carried the series still further. In one country the origin of the five-tone scale seems to have been forgotten, and the Octave now has five *equal* steps.

The ancient Greek scales are not certainly known in all respects, but the general statement is that the early lyre had four strings, the outer ones tuned at the interval of a Fourth, as A down to E, while the intermediate strings had various tunings; the important thing to notice is that the unit was not the Octave, but the Fourth, then called a Tetrachord. Later, other similar units were added to make one or two Octaves. This addition of Fourths lasted through the Middle Ages, and a curious survival of it remains today in the German notation; two Fourths up from C brings us to B-flat, but the Germans call the note B, and use H for our B.

The addition of unit-intervals, like our Octave or the Greek Fourth, may be carried out with smaller units; thus, an Arab rule is based on a lute or guitar with two strings tuned to give some small interval, perhaps a fraction of a tone; move a fret or bridge along under the string of lower pitch till its note is in unison with that of the higher; press the higher string at this fret and locate a new fret along the first string; and so continue as far as desired: the result will be a step-by-step scale of equal steps, and their sum may or may not chance to fill exactly some recognized large interval as a Fourth or Octave.

The most common principle of scale-building is based on a stretched string, called by the medieval writers a monochord: and every one knows how easy it is to produce a succession of tones by shortening the string, preferably by frets, as on a guitar. While ordinarily this successive shortening is made to give a harmonic scale, there are numerous old instruments that show, and various rules that direct, the placing of these frets at equal linear distances; hence arises still another scale,—one whose steps increase as the pitch rises; so if the

lowest step chanced to be C-C*, the step an Octave higher would be about *c-d*.

The same idea of equal linear spacing is very often applied to the holes in flutes, as one may see in any museum of instruments, and the resulting scale is of the same type. It may also be applied to lengths of vibrating bars, and in these the steps of the scale will increase still more rapidly.

One other principle of scale-building should be noted—that based on harmonics, which Helmholtz developed so admirably. It is familiarly known that a series of sounds can be produced from a simple bugle or trumpet, as C, G, c, e, g, etc.; these are called “harmonics;” the same thing is easily proved to be the case with a stretched string or wire; and it is also true that several of these sounds are ordinarily given off by the string simultaneously. If the lowest sound has, say, 100 vibrations per second, the others will have very nearly 200, 300, 400, etc., vibrations; if another string is furnishing simultaneously the series 151, 302, 453, etc., there are two sounds, partial-tones, as they are called, 300 and 302, that are nearly in unison but not quite; so they will beat and produce a roughness in the sensation; by flattening the higher string to 150 the beat will disappear and the ratio of the two vibration-frequencies will become exactly 2:3; the musical interval is that of the Fifth. Taking other pairs of notes in the same way, it is easily seen that with a few other ratios for the fundamentals, the harmonics will be free from beats, as 3:4 and 5:6; but with actual stringed instruments the largest numeral in any ratio is 9. If one tried to use this principle with stopped organ-pipes, the only numbers he could use would be 1, 3, and 5; in other words, the harmonic principle would not furnish a scale if one had only stopped organ-pipes. If he tried to work with bars the ratios of the partials to the fundamentals would be expressed in a complicated way, for the partial tones here are not harmonics. This principle of Helmholtz was probably not of much importance historically in the fixing of the old melodic scales; but it furnished an admirable justification of the scale in vogue at the date when he wrote.

Since every intelligent discussion of the scale has involved the use of numbers, it is interesting from a historical point of view to note that the Greek theorists (how they were led to fix on them can only be surmised) were content to work with 2 and 3, their products and powers; Zarlino broadened the tonal foundation by introducing 5, and therefore making more use of 6 than the Greeks did. Poole and others have insisted on using 7, though it upsets the diatonic scale; and Gevaert, Director of the Brussels Conservatory, asserted in 1883 that modern composers *do* use 7, giving the so-called natural Seventh, but do not use the next primes, 11 and 13. Of course passages for this Seventh must be scored for the horn, violin, etc., not for an instrument of fixed tone like the piano.

It may be recalled that a favorite way of defining the European scale for a century and more has been to consider it as made up of three Major Triads F, A, C; C, E, G; G, B, D; the vibration-frequencies in each case having the ratios 4:5:6. This is the "Just Diatonic Scale Major" of the theorists, often ignorantly and arrogantly spoken of as "the natural scale." A similar operation with the ratios 10:12:15 gives the harmonic minor scale, which does not agree throughout with the former scale. The series of Fifths will give a different scale; and modulation with any of those scales will introduce numerous new notes, nearly but not quite in unison with others. As harmonic music developed the complication became excessive and the discussions over the possible reforms became very acrimonious. In the early books of harpsichord music one finds the music confined to four or five keys; F, C, G, D, etc.; but Bach wanted a wider range of keys and large freedom for modulation; and as his favorite instrument was the clavichord (not piano, as some books say), with a delicate, short tone he found it practicable to tune this to equal-temperament, like our present pianos; so he could play in all keys, and in 1722 and 1740 he wrote his famous Fugues and Preludes. Later, when the pianoforte came into use, the fashion had been set, and as the instrument would not stand in tune long any-way, theoretical protests were scorned. If

Bach could have used a modern concert-grand piano, with its powerful long-continuing sounds, it is pretty certain he would never have advocated in preference to the other temperaments of his day the one called "equal;" and it took more than a century to bring the organists generally to accept that tuning. In our times occasionally practical instruments are made that provide for "Just" tuning in several keys; they are called "Enharmonic Instruments" and are usually made with reeds, which stand in tune much better than strings. It would contribute incalculably to musical education if a thousand such instruments were in use in this country.

The facts that have been given, and others for which there is not time, show that many principles have been used in actual practice to fix the notes of a musical scale; that most of these give results in close agreement for a very few intervals, but for the other intervals the results conflict; that there is no preëminently natural scale; that our scale or scales, as defined by theorists or rendered by the best violinists, or fixed by good tuners, reveal elements as diverse as the elements of our language or our population. Our scale does not correspond exactly to any theory founded on the laws of any vibrating bodies or of the ear; it is a compromise as every one knows, no interval, unless it be the Octave, having exactly the ratio of whole numbers; and even the higher Octaves of the piano are tuned sharp by every tuner I ever talked with. To further complicate matters the usual instruments cannot be kept in exact tune long, so the scales actually heard are quite uncertain.

There have been numerous schemes on paper for new scales; the newest one is in a recent British patent; here the Twelfth, C to g, is taken as the unit, and divided into 24 equal steps, each equal to nearly eight-tenths of a piano-semitone; so 15 of them make nearly a true Octave. Obviously the proposal is as arbitrary as one to introduce a new language, like Volapük, which has no literature of its own.

Returning again to non-European and primitive scales: Perhaps there is no blunder more common than to speak of Oriental scales as using very small intervals; if the scale

should have such steps the two notes are not taken in succession; our chromatic scale is ordinarily a store-house out of which a few notes are selected for use in a melody; and one can strike all the 12 keys in the Octave, yet use only the large steps of Fourths and Fifths. I have never met in Oriental music such a series of small intervals as the long chromatic passage in "Carmen," in the air "Love a vagrant is." On the other hand, it is well known that much Oriental and even classical music has a 7-tone scale with the large step of an undivided minor Third, three semitones.

Some relations of non-harmonic scales I have stated in more formal shape in a published paper and may be allowed to quote from it.

"The broad fact which underlies all study of scales was recognized by the Greek musician Aristoxenus three centuries before the Christian era. He pointed out that the voice, in speaking, changes its pitch by insensible gradations, while in singing it moves mostly by leaps. We recognize the same fact when we say that a singer follows a *scale*, but do not say it of a speaker. The one, to use the common figure, ascends or descends a ladder or staircase; the other follows a continuous slope, and may never step twice in the same place. Now, it is quite possible that in a song the voice may always move by leaps, and in repeating the song always take the same leaps as closely as can be observed, yet never strike a note which it has struck before; just as one may toss a stone up and down on a hillside, marking each time where it lands, and after a hundred tosses finds it had not landed twice at quite the same level, or in striding up and down hill may never plant his foot twice at the same level. I think this was the character of the songs of the first stage and of much primitive song today, though the evidence is too scanty to be conclusive.

"However this may be, it is certain that most peoples who have attained any moderate degree of civilization have attempted to limit the number of steps to be taken by the voice in any song between the highest and lowest note, and to fix these steps by rules, so that many men may learn them and

be in substantial agreement. Various old writers give the rules in vogue among Greek theorists; in the last century Amiot described the Chinese rules, while in the last few decades the rules of Arab, Hindu, Japanese, and Siamese musicians have been made accessible. The most familiar rules, as is well known, depend on that law of vibrating strings which is followed by a violinist in his fingering—namely, that the frequency of vibration of parts of any stretched string is inversely as the length of the parts, provided the tension does not change. Our latest rule, historically derived from one of the many Greek and Arab rules by subdividing the whole tone-steps, so giving twelve steps to the Octave, is embodied on the neck of a guitar or mandolin; here it is obvious that the successive stopping points as marked by frets get closer and closer together as the pitch rises. All musicians know that this number of notes, twelve, is found confusingly great for ordinary playing, and know the principles by which the player selects certain notes for any tune. But this multiplicity of notes has an important bearing on all studies on non-harmonic music made by harmonic musicians; for every sound within the compass of the instrument comes very near to some one of the twelve notes and may readily be represented thereby, owing to the difficulty the hearer has in estimating deviations from the familiar series and in noting them down. The results of this approximation are to mask all deviations from the twelve-tone piano-scale, whether intentionally or accidentally made, and to make it appear to musicians, first, that nearly all the music of the world is performed substantially in our scale; and second, that any other theoretical scales, such as those found among Orientals, or described by our European ancestors, are merely mathematical jugglery and of as little significance as proposals for a change that occasionally appear in modern musical or scientific journals.

“But before going further it must be recognized that the word ‘scale’ has many meanings. Perhaps the lowest and loosest is,—the series of sounds used in any musical performance, arranged in order of pitch. Another is,—the series of

sounds produced upon a particular instrument; while the most exact definition, but one applicable only where musical principles are well developed, is this:

"A scale is an independently reproducible series of sounds arranged in order of pitch, recognized as a standard, and fitted for musical purposes."

"While the last two definitions imply an instrument in which the scales are embodied, the limitation is in appearance only, for there is no evidence that any musicians do have a standard series of tones, unless they have one or more instruments embodying it, and have learned the series directly or indirectly from such an instrument.

"In the development of musical scales four stages may be recognized:

"1. The stage of primitive music, where there is no more indication of a scale than in the sounds of birds, animals, or of nature. Students of the origin of music may give free rein to their fancy in considering this period, and the uncertain musical utterances of living primitive peoples may be construed in accordance with almost any prepossession of the hearer.

"2. The stage of instruments mechanically capable of furnishing a scale. This stage has been almost entirely overlooked by students.

"3. The stage of theoretical melodic scales,—Greek, Arab, Chinese, Hindu, Medieval, etc. All the original treatises concerning these scales imply that a stage of development has been reached far in advance of the second.

"4. The stage of the modern harmonic scale and its descendant, the equally-tempered scale, which are alike dependent both on a theory and on the possibility of embodying it in instruments.

"These four stages, of course, overlap even in the same locality; they correspond in a rough way to the recognized four culture-stages, namely: the savage, barbarous, civilized, and enlightened."

Finally, it should be remembered that the musical scale is analogous to the alphabet, out of which words, phrases, and

paragraphs are built up. This analogy was the basis for the title of various old books, "Grammar of Music"; and it suggests that there are various independent scales, so that a musician versed in one musical language is not likely to judge intelligently of another without special training. Also it should be remembered that the knowledge of the scales is only a stepping-stone to the knowledge and the life of a people.

In a letter telling of an enjoyable visit from the young Felix Mendelssohn, Goethe exclaims, "Who can understand any phenomenon if he is not thoroughly imbued with the course of its development?" This course for our subject will become clearer as with the perfected field phonograph the study of non-harmonic music enters on a new stage, and results accumulate from the many workers now busy in all parts of the world. So one looks forward to the time when ethnologist and musician, historian, psychologist and physicist, all believing in evolution, shall coöperate in finding and telling the truth about the world's music; such truth will not only advance our science, but will widen men's sympathies and set them free from some errors, and so contribute to a liberal culture.

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